Popular Astronomy.

Vol. XIV No. 8. OCTOBER, 1906. Whole No. 138

ASTRONOMICAL PHOTOGRAPHY WITH REFLECTING TELESCOPES.

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FOR POPULAR ASTRONOMY.

If there is any limit to the service which photography can render to astronomy, there is no danger of its being reached. It is giving us comprehensive and imperishable records so accurate in themselves that the precision in the result obtained finds a practical limit rather in our skill in measurement and interpretation than in the errors of the plate itself. The present paper will deal with two phases of astronomical photography, neither of which are new, but which have not had the attention they deserve, at least in popular astronomical literature.

The first point is the photography of mixed regions containing both stars und nebulæ, illustrated by figures 1 and 2 of frontispiece showing the wonderful and often-described Great Nebula in Orion. The negatives were taken by the writer with the 24-inch reflecting telescope of eight feet focal length, constructed by Ritchey and mounted in the southeast dome of the Yerkes Observatory. Figure 1 is from a negative exposed 1905 March 8 from $7^h\ 9^m$ to $7^h\ 19^m$ central time, with the aperture of the mirror reduced to twelve inches. Even with this short exposure and small aperture the star disks are quite intense, and fine details are shown in the outlying parts of the nebula. But the brighter parts of the nebula are overexposed and the faint stars near the trapesium entirely invisible. All of these faint stars have been suspected of variability by such good observers as Bond or Struve.*

Hence it has become a matter of considerable importance to secure photographic records yielding results uneffected by the great difficulties which beset the visual comparisons of these stars, arising from the bright background of the nebula, which flickers with the least unsteadiness in the atmosphere and renders estimates of magnitude extremely uncertain. In order to photo-

* Compare *Astrophysical Journal*, XX, 137, September 1904.

graph the faint stars imbedded in the nebulosity, a filter must be found which will be transparent to the star light but opaque to the light of the nebula. The nebular light is mostly concentrated in the bright lines at wave-lengths 5007 and 4959, known as the first and second nebular lines, the lines of the hydrogen series, and the ultra-violet nebular line at wave length 3727. The star-light, on the contrary, is well distributed along the continuous spectrum, with a maximum of photographic action in the blue. A filter was made which transmits only a band in the blue between the second and third hydrogen lines, the limits being about wave-lengths 4860 and 4640. The problems arising in the construction of such a filter were very successfully solved by Mr. R. James Wallace, photophysicist of the Observatory staff, whose familiarity with the photographic action of dyes enabled him to make a dry filter, which, placed nearly in contact with the sensitive plate, gave the result shown in figure 2. This negative was exposed 1904 November 18 from $11^h\ 55^m$ to $12^h\ 25^m$, central time, with the full aperture of the reflector. As the light from only a short strip of the spectrum was used, the exposure time required a considerable increase, and the star images are not quite as intense as in figure 1, but the contrast in the nebular light is striking. On the negative itself the faint stars near the trapesium are well shown, yielding an extremely valuable record of the light-intensities.

Almost boundless are the possibilities opened up by a skillful combination of light-filters and plates sensitive to the corresponding colors, but only a reflecting telescope will utilize them completely, as no other form will focus all the rays on the plate. For example, attempts have been made to compare the proportion of white and colored stars at different galactic latitudes, using orthochromatic plates and a portrait lens. The negative results obtained evidently have a sufficient explanation in the imperfect correction of the lens for yellow rays. The light-filter enables us to study the distribution of radiations of chosen wave-lengths from any celestial source. Stars too faint for direct spectroscopic study can thus be made to yield the secrets of their constitution to the photographic plate.

Figures 3 and 4 illustrate the second part of this paper, and relate to the use of the photograph as a means of obtaining precise star places. It is now generally admitted that the photographic results equal or exceed in accuracy the best micrometer or heliometer measures, in spite of the fact that the star images on the plate are many times larger than in the telescope. To

obtain a numerical comparison of these diameters, take for example a 24-inch telescope of 36 feet focal length. By Dawes' formula the diameter of the central diffraction disk is 4″.5 divided by the aperture in inches, or 0″.2, equivalent to 0.011 mm in linear measure. As compared with this very minute quantity the diameter of the smallest disk which is fully exposed so as to give a well-defined edge, is about 0.050 mm on the 24-inch reflector plates. One would naturally suppose that the micrometer wires could be set with greater accuracy on the small visual disk than on the large photographic disk, but as a fact, the contrary is nearer the truth. Among the difficulties encountered by the visual observer the following may be mentioned: wandering of the image on account of imperfect running of the driving clock; interruption of measures by clouds, etc; fatigue caused by the effort to maintain himself in a strained position before a moving eyepiece; but worst of all, especially with a large telescope, the star image flickers, wanders and even "jumps" on account of the unsteadiness of the atmosphere. Figure 3 is an automatic record of such wanderings. The telescope was at rest and some of the stars of the Pleiades group were allowed to trail across the plate. In a perfect condition of the atmosphere each trail should be a uniform straight line. The record of the seeing was "clear, unsteady, no wind," so that the irregularities in the trails were not caused by any shaking of the telescope, but were entirely atmospheric. A close inspection of Figure 3 will reveal several interesting points. The cut is a 20-fold enlargement from a negative exposed 1904 December 13 with the aperture of the reflector reduced to 12 inches. The portion of the trail included in the cut was traversed by the star image in 12 seconds. The heavy trail is overexposed and considerably smoothed in the process of reproduction, yet it shows distinctly the crooks caused by wanderings across the direction of motion. The lighter trail also shows the alternate strengthening and weakening caused by irregular motion of the image in the direction of the trail. These condensations have a period roughly approximating half a second in the case of this trail.

The task set before the visual observer with the micrometer is evidently a difficult one. He must either follow with the wire the wandering and jumping image, or else he must estimate its mean position. Either method will lead to considerable accidental errors, which, combined with those from the other sources above mentioned, explain the relatively low degree of accuracy in visual measures.

Figure 4 is enlarged 2.7 times from a 4-minute exposure made eight minutes before the trails of Figure 3, so that the atmospheric conditions were the same. It includes Maia, Taygeta and Electra of the Pleiades group, the last named star, at the upper right-hand corner of the cut, being the same one which formed the fainter trail in Figure 3. In this exposure the telescope was moved by the driving clock, and the wanderings of the image, so troublesome in visual work, are integrated to form a round disk, which can be bisected with great ease and accuracy under the microscope. This particular exposure was too long for the three brightest stars shown in the figure, so that they begin to show the square form caused by diffraction effect of the arms supporting the flat mirror. In practice the exposure would be so timed as to give round images of the stars which are to be measured. A comparison of Figures 3 and 4 gives quite a vivid impression of the difficulties encountered in the chase after the nimble image in visual work, as compared with the ease and accuracy of bisecting the stationary round image on the photographic plate, and offers at least a partial explanation of the greater precision of results from the latter method of work.

Yerkes Observatory,
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THE EFFECT OF TEMPERATURE UPON THE CONSTANTS OF A TRANSIT CIRCLE.

HERBERT R. MORGAN.

[Communicated by Rear-Admiral Asa Walker, U. S. N., Superintendent of the U. S. Naval Observatory.]

The variations of the constants of the 9-inch transit circle of the U. S. Naval Observatory, depending upon changes in temperature, have been, to some extent, investigated formerly. The present investigation was undertaken at the suggestion of Professor W. S. Eichelberger, U. S. N., to whom I am greatly indebted for valuable suggestions and kind encouragement.

The present method of observing, inaugurated in September 1903, by Professor Eichelberger, furnishes numerous sets of constants during an observing day. A list of stars two or three hours long is preceded and followed by a complete set of constants; and each isolated object as the Sun, Moon, or a planet, is provided with a set, furnishing as many as four or five sets during some nights, and two or three during the day. A fairly continuous record of the instrument is thus kept in clear weather.

The azimuth constant is derived from readings on the north and south meridian marks, the constant positions of which, for a few weeks at a time are derived from numerous pole star observations. The variation of these adopted positions of the north mark in the three years September 1903 to May 1906 was only $0^s.089$; that of the south mark $0^s.130$; the probable error of these adopted mean positions for a few weeks being about $\pm 0^s.004$. The level constant is derived from measures on a fixed wire, and its image in mercury, while the instrument is set to determine the nadir point. From October 1904 to June 1905, 515 readings of the hanging spirit level were made in addition to the mercury level, the monthly means of the differences in the two ranging $0^s.018$.

The collimation constant is the mean of two independent determinations; one from measures on the collimators, the other from the measures on the marks. Their average difference for the year 1905-4 was $0^s.003$.

During the year September 3, 1903 to August 16, 1904, the year used in this discussion, there were 938 observations for azimuth; 948 for level; and 914 for collimation. The azimuth varied $0^s.850$, the level $0^s.650$. The collimation was necessarily adjusted a number of times, destroying its continuity. Fortunately the necessity for such frequent adjustment has not recurred.

Azimuth and Level.

An examination of the azimuth and level was made in five ways.

First. Twenty-three groups arranged according to *time* were taken, with their temperatures; making each group about two weeks long, and embracing 30 to 70 constants. From the means of these groups the annual and monthly terms were formed. The results were the same as those obtained by considering the day and night values separately. The annual terms are:

$$da = -0^s.009\ dt$$
$$db = -0\ .008\ dt\ ,$$

where t = temperature; a = azimuth; b = level. This is shown

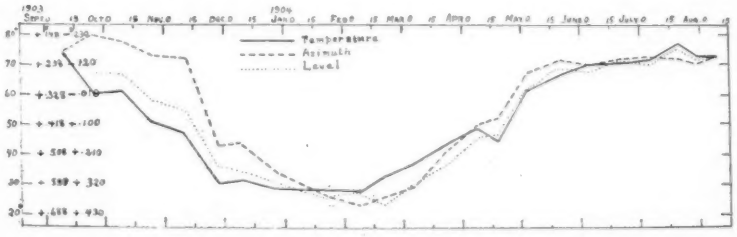


Fig 1

graphically on Figure 1, where the scales are $da = -0^s.011\ dt$ and $db = -0^s.009\ dt$. By taking groups in summer and winter, the annual terms for both 1904-5 and 1905-6, are:

$$da = -0^s.014\ dt$$
$$db = -0\ .010\ dt.$$

Second. Of the above groups, or combinations of them, three to five sub-groups, of about 20 constants each, were formed, arranged according to *temperature* in each group. As for a week or two the temperatures at the same time of the day would run about the same, these sub-groups would, in a general way, correspond to different times of the day, and between them give daily terms, thus:

For $t <$ 30°	$da = -0^s.008\ dt$,	$db = -0^s.004\ dt$
" 30 — 40	" $= -0\ .010$ "	" $= -0\ 004$ "
" 40 — 50	" $= -0\ .005$ "	" $= -0\ .005$ "
" 50 — 60	" $= -0\ .005$ "	" $= -0\ .005$ "
" 60 — 70	" $= \ \ 0\ .000$ "	" $= -0\ .004$ "
" $>$ 70	" $= +0\ .001$ "	" $= -0\ .002$ "

Herein the coefficient itself is shown to be a function of the temperature. This has further proof.

Third. The short period terms of the previous step were checked in the following manner: on 109 days during the year when constants were determined frequently during a period covering twelve hours, or more, the means of two or three sets around midnight, gave, for each day, a zero point for each constant, from which all values of that constant for the day were subtracted. The means of the differences at each tenth of a day were plotted, as in Figure 2.

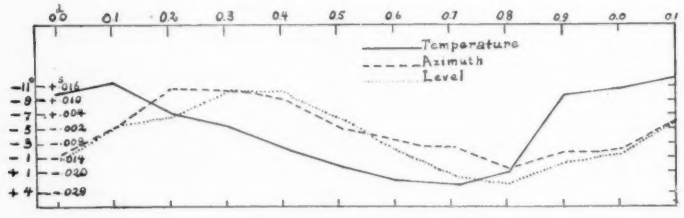


Fig 2

Fourth. At eight points during the year, the temperature curves from the thermograph sheets were plotted, for a few days, together with the individual values of the level and azimuth. One of these is shown in Figure 3, where we notice the level and azimuth oscillating with the temperature for five days in May 1904. This is characteristic of these plots.

Only a glance at the plots is needed to show that the instrument

does not at once respond to a change in temperature. There is a very evident *lag* which in the yearly swing may be a fraction of a month. The maximum or minimum points of the daily curves show that for such periods this lag is a few hours, this being, however, a function of the amount of change in temperature, and to some extent of the temperature itself. The average of those actually plotted is 3.5 hours. The "tenths day" curve in Figure 1 gives four to five hours. The less lag from a minimum temperature is noticeable. The lag of the instrument being real, the results of the first three steps are, therefore, more or less

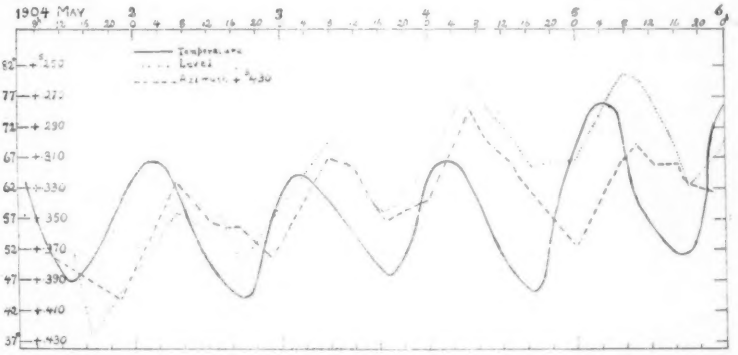


Fig 3

vitiated, as, in them, the simultaneous temperatures were used. In deriving a coefficient one should use the effective temperature at the time the constant is given, which, in this case, owing to the lag, is the actual temperature of a few hours preceding that time. This was done as follows:

Fifth. Three groups of constants, continuous in themselves, were taken at random, viz: 88 in April 1904; 58 in June 1904; and 52 in November 1905. In these groups, the probable error of a single determination of a constant was found upon different suppositions; ie., of no temperature term; then of a temperature term, and no lag; and finally with lags of three to five hours, using different temperature coefficients. In doing this the temperatures were taken from the continuous record of the thermograph sheet. The numerical results for November 1905 are:

Hypothesis	Level prob. er.	Azimuth prob. er.
$(b - b_0)$,	$\pm$ 0^s.031	$\pm$ 0^s.040
$(b - b_0) + 0^s.003\ (t - t_0)$,	$\pm$ 0 .029	$\pm$ 0 .034
$(b - b_0) + 0\ .004\ (t - t_0)$,	$\pm$ 0 .029	$\pm$ 0 .034

$(b - b_0) + 0.003\ (t - t_0 - 5\ dt)$,	$\pm$ 0 .024	.....
$(b - b_0) + 0.004\ (t - t_0 - 5\ dt)$,	$\pm$ 0 .021	$\pm$ 0 .027
$(b - b_0) + 0.005\ (t - t_0 - 5\ dt)$,	$\pm$ 0 .021	$\pm$ 0 .026
$(b - b_0) + 0.006\ (t - t_0 - 5\ dt)$,	.....	$\pm$ 0 .028

"5 *dt*" represents the change in the temperature in the five hours preceding an observation. The drop in prob. er. with the introduction of this term is conclusive.

For April 1904 the smallest prob. er. came from the hypothesis

$$(b - b_0) + 0^s.003\ (t - t_0 - 5\ dt),$$

the assumption of a lag of four hours satisfying the observations nearly as well. For June 1904, when the temperature was 70°, the level and azimuth gave the different results,

$(b - b_0) + 0^s.003\ (t - t_0 - 5\ dt,)$ and $(a - a_0) - 0^s.001\ (t - t_0)$

This corroborates what is shown above, ie., that the azimuth term is very small in hot weather. This last step, also, leaves no doubt but that there is a daily variation of the instrument in azimuth and level, arising from temperature variation, and that, under normal conditions, the effective temperature at a given time is the actual temperature four or five hours preceding.

Further details cannot have room here. Enough is given to show that there is a well established motion of the 9-inch transit circle in azimuth and level, which is a function of the temperature; that this motion is not the same for azimuth as for level, but is itself a function of the temperature, especially in azimuth for which the motion is larger in winter than in summer; and that the instrument lags behind the temperature an amount proportional to the change in temperature. A time change has not been shown. The question as to the variable relative change in the piers in the two coördinates is relevant, and seeks a physical explanation, as yet, not found. In practice the annual term is not used; as however, three years have proved the swing of the instrument with the annual temperature, more confidence is had in the terms of shorter period. Moreover, in a short period the lag prevents a given change in temperature becoming fully effective before a counter effect sets in, and such terms should decrease with the period. This is the case. Nor is the question begged in applying these terms to justify individual variations in the same constants, which, collectively, have given such terms, for accidental errors have been eliminated. Further the terms derived from one year's work have fully satisfied the work for three years, as all the levels and azimuths for the years September 1903 to May 1906 have been examined, and all variations, or apparent discrepancies satisfactorily explained by these terms. An example at random, in February 1904, is given.

Interval in days	*a*	*t*	*da*	*dt*	(*da*)
	+ 0ˢ.400	30			
4.4			+ 0ˢ.150	− 10	+0ˢ.040
	+ 0 .550	20			
1.8			− 0 .158	+ 17	−0 .005
	+ 0 .392	37			
0.3			− 0 .106	+ 9	−0 .043
	+ 0 .286	46			

In the first interval the temperature dropped to 0°; a large change, and a longer interval: the large coefficient −0ˢ.011 was taken from table. The other intervals decrease, and the temperature increases so −0ˢ.009, and −0ˢ.007 were used. The last column (*da*), where the computed change for temperature has been eliminated, is considered satisfactory. It is not possible to choose temperatures closely in such an examination.

COLLIMATION.

The processes of examination applied to level and azimuth were previously applied to the collimation. Owing to adjustments, the longest period of continuity was seven months, from cold to warm weather, and as the temperature increased with the time the effects of the two could not be differentiated, either giving $dc = + 0^s.004\ dt$. Discussions of this instrument in former years have given $dc = 0^s.003\ dt$, and $dc = 0^s.004\ dt$; the sign changing with the reconstruction of the instrument in 1892, and is now positive for clamp west. In 1904-5, and 1905-6, when the collimation was undisturbed, the annual term was $dc = 0^s.004\ dt$. No daily term of this size exists. The prob. er. method in April 1904 gave $dc = + 0^s.001\ dt$, and for July $dc = + 0^s.002\ dt$ and the plots give similar results. The variation in cold weather seems smaller. The arrangemnt with regard to temperature gave

For t $< 40°$ $dc = 0^s.000\ dt$ to $dc = 0^s.001\ dt$.
" $> 40°$ $dc = +0.001\ dt$ to $dc = +0.002\ dt$.

At some points, evidence is afforded of small unexplained jumps in the collimation, independent of temperature. They do not exceed 0ˢ.050, and variations in three or four hours seldom reach 0ˢ.030. The instrument may be a few days behind the annual temperature, but no daily lag is detected.

CONCLUSION.

Descriptions of the 9-inch transit circle are found in the volumes of the observatory. The piers are marble, and set on capstones of granite resting on a massive concrete foundation. There is a minimum amount of metal supporting the wyes, at two holes in the piers. Temperature variations of the constants of different transit circles have been discussed by Professor Updegraff, Pro-

fessor Hough, and others. While the non-existence of such variations is a convenience, their existence need not vitiate the work of an instrument, but an accurate knowledge of the laws of such variations is essential to the proper reduction of the observations.

YERKES OBSERVATORY.

W. W. PAYNE.

During the summer vacation it was my privilege to accept an invitation very kindly given by Professor E. E. Barnard some time before the close of the last college year, to visit the Yerkes Observatory at Williams Bay, Wis.

I was most delightfully entertained by Professor and Mrs. Barnard for a whole week, in their own beautiful home, just a few minutes' walk from the Observatory building, and so near to Lake Geneva that the wide outlook over the charming inland lake was a pleasure to me that has been equaled very rarely during my whole life. Their new home there has such comfort, modern taste and simple beauty outside and in, that one can not wonder that many of the noted scholars of the world have visited it, and left with its worthy possessors tokens of appreciation and high regard that the best of earth might rightly covet and enjoy.

Next to a vacation rest, in this one of the many beauty spots around Lake Geneva, in interest, was the great Yerkes Observatory which, by the courtesy of these dear friends, could be visited leisurely and more thoroughly than ever before. Only one such privilege had come to me before, and that was through the kindness of Professor George E. Hale, the former director of the Observatory, on the occasion of its dedication about ten years ago. At that time were gathered at Williams Bay a large number of representative astronomers from our own and some foreign countries to participate in a program carefully prepared for such a notable occasion. While the Observatory was then already open and some of its large instruments at work, many other important lines of investigation were not yet begun. Everything was new. The astronomers then belonging to the Observatory were busy, day and night, with the exacting duties of installation of instruments and the task of finding constants for the necessary reductions of practical work. While all present on that occasion were interested in what they could see and know about the Yerkes Observatory at its formal opening, large interest was felt apparently in the distinguished scholars that

met there and so freely participated in the daily exercises and discussions of the conference. The way those great astronomers handled the live questions of the day in the newer astronomy made deep and lasting impression on the minds of those present. It was truly an occasion to be remembered for the benefits conferred on astronomy in particular and on all science related to it.

In my late visit to the same place again, it is seen how well and admirably the auspicious opening has been carried forward and developed and amplified in all the various lines of research work associated in what we now call the New Astronomy.

In the first place, the Yerkes Observatory is very fortunate in having, at its head, as Director, Professor E. B. Frost, a man amply qualified for the large and conspicuous place to which he has been called. His work in spectroscopy is widely and favorably known, and we were interested to meet him and have so full an opportunity to see and know about his work generally, and that of the motion of stars in the line of sight particularly. With the great forty-inch telescope and its effective appliances at hand, Professor Frost is equipped for work in these lines that few elsewhere can do on account of the lack of adequate instrumental aid. In looking over the work he is now doing, and noticing the exact, neat and clear method he has adopted for his records, we were very favorably impressed with the simplicity and the completeness of it all either for present uses or for future reference at any time. As far as we know and can judge of its merits it is certainly a model of its kind. The work of spectroscopy which is now carried on by the aid of photography is intensely interesting in its recent developments. It involves so much of detail in the study of spectrograms that the astronomer must know well the character and the behavior of the shifting lines of the spectrum due to the motion of a star, as well as the exact meaning of the way the sensitive plate should work for his record in order to get the decisive results of which he is in search. The points yet unsettled in regard to principle in both spectroscopy and photography offer some difficulties that are hard to overcome. In this important field of work Professor Frost has an invaluable aid in the skill and knowledge of R. James Wallace who is an acknowledged expert in photography and the varied means of its reproduction. We were in his office for a few minutes only before his vacation time came, and so were not able to see as much of his instrumental methods and the lines of his present work, as was very greatly desired. We were gratified to learn that Mr. Wallace has been given a permanent place in the

Observatory staff and that the important work he is now doing will go on uninterruptedly.

It was my great privilege the first night that I reached Williams Bay to go with Professor Barnard into the dome of the great 40-inch equatorial of the Observatory, and to stay there with him until the small hours of the morning, on one of those nights good for astronomical observation. When such nights come, the working astronomer is about the happiest man that lives, for then the stars come very near to him in all their beauty and real splendor, and it seems to him that the oldest friends he has among them bring to him something new every time he looks. If he has a mind or a heart in him, he can best wonder at and admire continually the handiwork he sees profusely strewn in the star depths all about him.

I looked at several of the Messier clusters, Mrs. Fleming's variable 1898, (very faint), the cluster in Perseus and several other objects including the Ring Nebula of Lyra. In this last object I could see the faint star within the ring which is near its middle point. I had previously tried to see this very faint star in other large telescopes, but was never able to do it before. Possibly the conditions at other times may have been less favorable or, possibly the central star may be slightly variable.

In the great retractor the ring itself stood out in view splendidly, with more of detail than I ever saw before. It was a grand sight to one already somewhat familiar with it.

That night was one of great profit to the writer in not only what he saw, but especially, also, in the help it gave him in comparing the work of a very large telescope with that which smaller ones can do under the same conditions. So much has been said by persons of influence in science about what the great telescopes of the present time can or can not do, as compared with those of lesser size, that I was greatly interested in looking at various objects, at this time, for the purpose of getting a practical impression in my own mind, in my own way of thinking as to the real power of that great telescope. I do not refer to my way of thinking and my impressions as better than those of others. Far from it. Many others doubtless would be better than mine; but still it must be true that a judgment that is worth anything must be, in a large degree, one's own obtained in some such way.

In this one night, the impressions received regarding the work of that 40-inch instrument are clear and vivid. I feel quite sure of a few things about good large telescopes when compared with

smaller ones that are equally well made. One is, that the same person will see more and see better in a large glass than he possibly can in a smaller one, equally good in make, and kind and place. Another is that different persons observing on the same instrument independently, often reach different conclusions in degree, if not in kind. The reason of this is probably that different persons have different degrees of skill in seeing, comprehending and fully relating phenomena that they mentally pass upon. In this reference we do not mean the work of those observers that let their imagination run away with them into wild and observed theories, but only those who work and think very carefully and very thoroughly, in bringing severe tests to method and conclusion required by the strict laws of scientific logic which should always govern a scientific imagination.

In the Messier clusters Mr. Barnard would see much more than I could and when he would call my attention to certain faint wisps of nebulous matter found in different parts of some of these clusters, I could see much that he would suggest—not all—for his eye is better than mine; but that which I could see, and had not noticed before, I was sure of because I could test my eye in my own way and have assurance follow.

Mr. Barnard is now triangulating the important Messier clusters and, of course has made a long and very careful study of these beautiful clusters. In his eye-estimates of the magnitudes of the stars of these clusters when he is getting their exact distances from his reference stars, he has noticed that several of the stars in the different groups are variable. His practiced eye will detect changes of magnitude that are only a fraction of one unit of light measure in this way. This is certainly exact work with the eye, especially in the fainter magnitudes. It was in this field where the great Herschel went so far wrong in his judgment, though in almost everything else he was surprisingly exact, by the test of modern methods.

Much more ought to be said about the ability of the man behind the telescope, and what such an able one can do for science; but we must not digress too much. Suffice it to say, the able man behind the big telescope is more important than the mightiest instrument that can be made. If size only be thought of, make the ratio of lengths inverse and one will have an approximate value, or a working hypothesis in the direction of a true ratio.

As a third thing, the place where an observatory is located astronomers think is very important, and this is doubtless true in a large sense. This we know from experience gained by

observations made at stations in high altitudes, especially if such localities are not disturbed too much by air currents which are destructive to clear and steady seeing. It is also true that lower altitudes are found where favorable opportunity for observation is found some of the time of every year. It is doubtless true that lower altitudes are found where favorable opportunity for observation is found some of the time of every year. It is doubtless true that the higher places are better more of the time than the lower ones, because of less depth of atmosphere to be disturbed, or to look through, but, we are sure that the lower places are prime some of the time, and it behooves the observer with small or large instrument to watch faithfully for these rare times, and to use them for all they are worth when they come. We have had some nights in Northfield when the seeing was simply superb, in the 16-inch refractor. One could trace easily faint details in nebulæ and surface markings on the planets and measure double stars up to the theoretical limit of the object-glass.

Much more could be said, in detail, about these three important parts of astronomical work, and of their relations, as suggested by the comparison of large and small telescopes for efficiency in prosecuting the varied work of modern astronomy, but, enough has been indicated to show how important are the man, the telescope, and the place for work, when the very best results are needed for the advancement sought.

In looking over Mr. Barnard's recent work in photographing the Milky Way, and his micrometrical work in triangulating the Messier star clusters the thought of the observer, the instrument and the place, or site for observation were strongly uppermost in my mind.

It is known to many of our readers, that Mr. Barnard has recently procured a fine, large photographic telescope, especially adapted to work on the nebulæ. A full description of this new instrument will be found in Vol. 21, No. 1, of the *Astrophysical Journal*. After long and painstaking study Mr. Barnard planned and had made for him, an instrument for photography that possesses a high degree of efficiency as shown by the grand work it has already done in the hard fields of the Milky Way. When one remembers that the star-depths in the regions of the Milky Way are so far away that it takes light hundreds of years, if not thousands, traveling at the rate of 186,000 miles per second, to reach the Earth, we wonder that any telescope man can devise will have power enough to bring the human mind within seeing

and reasoning distance in such far off spaces. After a close and searching look at those superb positions of some of the interesting fields of the Milky Way, which showed much more than we have seen before by the aid of photography, we spent considerable time in examining the new Bruce Photographic telescope, a model instrument in design and finish, as Messrs. Warner and Swasey, the makers, know how so well to accomplish. When Mr. Barnard told me the problems that confronted him, in planning just such a photographic instrument as he must have to do the splendid work waiting for some one to do, we were not surprised that the task he set for the optician and other makers was a very difficult one, and that it taxed their utmost skill to get the results he wanted. We are sure our readers will be pleased to know that Mr. Barnard will soon give in this magazine some of the important results he has secured by the aid of this new instrument while he was on Mt. Wilson in California for the purpose of using it in the clear atmosphere of that admirable place for astronomical observation.

At another time we hope to give some account of Mr. Barnard's work in triangulating the Messier star clusters and to speak of what Mr. Parkhurst is doing in photometry and variable star studies. His field of work is intensely interesting and most practically useful to astronomy in many ways.

(To be continued.)

THE SUN AS A MAGNETIZED SPHERE.

FRANK HAGAR BIGELOW.

The radical change, gradually brought about by numerous physical researches in our ideas regarding the ultimate constitution of matter, by which we conceive of atoms as composed of dynamic structures of balanced rapidly rotating electric charges, has, also, materially modified the attitude of mind in which we approach the problem of the magnetization of large rotating heavenly bodies. When the atoms were regarded as hard, highly elastic, nonelectrical, and nonmagnetic spheres moving by the laws of kinetic energy in straight paths, except for the effect of collisions, it was of course difficult to prove that the Sun could be magnetic of itself at the high prevailing temperature. But if the integrated forces are to be derived from electric charges in very rapid motion, and this seems to be the case, then magnetic fields are essential to the existence of every kind of matter

whether in large or in small masses. Under ordinary conditions the primitive motions of the ionic charges are highly disorganized since the motions are in every conceivable plane, and the molar or large masses do not show any outside field of force. It is, however, only necessary to organize these ionic motions relative to one plane, that is, to polarize their orbits, in order to produce a common magnetization within the mass and magnetic field outside this body. When a steel magnet is subjected to heat the field is destroyed, simply because the minor magnets, as of the atoms and molecules temporarily oriented to one axis, have been made disordered by the higher class of collisions which has been induced by the increase of the temperature. This experiment, however, by no means exhausts the natural conditions of the problem. In a rotating body like the Sun the angular velocity induces a deflecting force at right-angles to the momentary motion, of every particle, $l = \omega q \cos \theta$, depending on the angular velocity, and polar distance velocity in any linear direction. When q is great, and in the case of the negative ions combined in the atom, it may approach the velocity of light, l is also a comparatively large force, so that a tendency to run into vortex motion is ever in operation. The entire mass of the Sun, by the general laws of motion, also is evidently organized as a great vortex about the axis of rotation, and these two impulses doubtless tend to polarize the motions of the individual atomic ions in planes perpendicular to this axis, or to produce magnetization nearly parallel to the axis of rotation. Since the movement of the negative ions is in excess of that of the positive ions, the resultant magnetic field will be due to this excess of the action of negative electric charges above the positive charges, since for a common direction of gyration the positive and negative charges produce oppositely directed magnetic field. Finally, if the negative charges, in the atomic conflict, tend to escape from their electric bonds and to roam in disregard of any structure, then these negative charges may well tend to accumulate as an electrostatic layer on the surface of the sphere. At least the negative body-charge may, as a whole, be located farther from the center of the sphere than the positive body-charge, and this will produce internal magnetization and surface electrostatic charge, such as the Earth possesses, and such as we have evidence to indicate the Sun also possesses. This dynamic view of mine comes to the same result as the static theory explained by Sutherland.* With

* A possible cause of the Earth's magnetism, W. Sutherland, Terr. Mag. June, 1900; September, 1900; December, 1904.

this introduction I shall merely mention various results obtained in my researches, which are at least circumstantial evidence that the Sun is a highly organized magnetic body, and that the numerous variations in its internal action constitute the causes for the observed changes in the external magnetic field.

(1) The polar rays of the corona, at least at minimum, conform to the normal lines of magnetic force on a sphere seen in projection and concentrated in a ruffle at some distance from the pole.

(2) The ions in the solar atmosphere are luminous along rays of magnetic force.

(3) The coronal poles are located asymmetrically with respect to the axis of rotation and persist from one eclipse to another in the same relative positions.

(4) The Sun's external magnetic field, as measured at the Earth, is so arranged that it has greater intensity in some solar longitudes than in others.

(5) By using large masses of observational material, the typical curve, attributed to difference in solar output in longitude and depending for its existence upon the structure of the Sun's nucleus, has been found to be reproduced approximately, in the distribution of the sunspots and prominences in longitude, in the terrestrial magnetic elements, the electric field, the barometric pressures, the temperatures, and the local cyclonic movements in the United States.

(6) There is evidence that the periods of rotation in the higher latitudes of the Sun fluctuate about a mean value synchronously with the external energy variations, showing that the entire external system is a direct effect of the forces producing at the same time the interior circulation in the mass of the Sun.

(7) The inference is that the Sun's magnetic field embraces the Earth, and reaches it in lines perpendicular to the ecliptic, falling upon the polar regions of the Earth along the planes of the magnetic meridians.

The electromagnetic radiation of the Sun transports to the Earth the other kind of energy which is concerned in the temperature excess prevailing in the tropics over that in the polar zones, and produces the observed general and local cyclonic circulations. The coronal rays, especially in the equatorial belts of the Sun, indicate that there are other forces in operation in the neighborhood of the photosphere besides those already mentioned.

(1) The spreading of the great streamers away from the plane

of the ecliptic suggest an electrostatic repulsion.

(2) The streams of ions, under the action of the mechanical pressure of light, move not in radial lines, but in curves as determined by the additional magnetic and electrostatic forces prevailing in the surrounding space.

(3) It is impossible to assert that enough ions reach the upper strata of the Earth's atmosphere to produce the observed variations of the terrestrial-magnetic and electric fields as registered in the lower strata, and it is not probable that this is the fact.

(4) This radial radiation of the photosphere may be to some extent variable in its output, and so produce seasonal climatic temperature and weather variations synchronous with it, as registered in the pressures and temperatures in different regions of the Earth.

(5) The normal equilibrium of the Earth's atmosphere is probably disturbed frequently, from day to day and hour to hour by the interplay of this complex system of correlated forces.

The problem of discussing the effects of the solar radiation upon the magnetic and the electrical fields of the Earth's atmosphere, and their relations to the meteorological elements, has been greatly simplified by the results of the research contained in the first four papers of this series. It has been shown that a different correlation of the quantities in consideration can be made and that in this way the intractable conditions which have so long puzzled scientists are decidedly ameliorated.

(1) The fact that there is no one synchronism common to the entire Earth between solar and terrestrial causes and effects, has been explained by showing that the temperatures synchronize directly in the tropical zones, but only inversely in the temperate zones, in consequence of the inverting effects of the general circulation; and that while the pressures in the Eastern Hemisphere respond directly to the solar impulse, they surge inversely to it in the Western Hemisphere. Similarly, the precipitation and the local circulation will have to be distributed by regional conditions in the final interpretation. This will reconcile much data that are apparently in conflict as evidence regarding the existence of synchronism generally.

(2) The discovery of ionization in the gases of the atmosphere, generated probably by the short-wave radiation, and the determination of the several types of the temperature waves in the lower strata of the atmosphere lifts the veil from the problems of the diurnal barometric waves, the electric potential

gradient, and the rate of change in the electric charges. These seem to be direct consequences of the temperature acting upon the density of the air in the different strata, and upon the locality, whether warm or cold, sought out by the ions.

(3) The cause of the *hourly* variation of the magnetic field is plainly shown to reside in the movements of the ions from one level to another. The cause of the *daily* variation of the magnetic field is probably in large part due to the movement of the ions from one hemisphere to the other, which at the same time produce the auroral displays simultaneously in each hemisphere, and the electric earth currents and local effects in the several portions of the circuit. This change of view relieves us of the difficulty of making the Sun the source of all the energy displayed in a large magnetic storm, since the initial impulse is due to that portion of the energy disturbing the normal terrestrial equilibrium, while much of the observed effect is due to the motion of the ions in a closed terrestrial circuit. It remains to discover in what proportion the energy should be distributed among these three sources, the polar magnetic field of the Sun, the variable radiant energy, and the terrestrial ionic circuit.

(4) It is now apparent that in using, for the basis of my original research, the magnetic field of the Earth as a register or solar pulse recorder, I have been amply justified in tracing out through it as the intermediary the synchronism between the solar surface variations, shown by the spots, faculæ, and prominences, and the temperature and pressure effects at the Earth, because it is in fact an intermediate effect, and evidently the most sensitive one with which we have to deal. The subtile influences of the invisible solar radiation may be registered in several ways, as bolometric spectrum curves, as actinometer integrations, as visible energy spectra, or they may be recorded as elastic potential effects and as magnetic force vectors. The latter are the most persistent in all kinds of weather, and most available for continuous observations. It is only necessary to determine what the connecting functions are in terms of magnetic force, to infer from the magnetic intensity what are the temperatures within two miles of the ground, in the midst of the cyclonic actions, to estimate the movement of the ionic currents, and to determine the relative amount of the incoming solar radiation, and thence to learn much regarding the variable nature of the circulation within the rotating mass of the Sun.

(5) It is now easy to see that several lines of scientific inquiry as to the period of the solar rotation have been misdirected.

The attempt to associate magnetic storms with the solar spots has failed, because the effective surface radiation on a given meridian of the Sun may or may not be associated with a large spot, which clearly depends upon the internal circulation for its existence. B sides this, the magnetic storm is in part dependent upon distinctly terrestrial conditions. The application of Schuster's periodogram to the magnetic declination, in order to determine the periodicity of the solar residuals, was incorrect for this reason. While the magnetic declination varies with the season of the year, and from one year to another, as a consequence of the solar radiation, this component is really a term in the hourly vectors only, due to the vertical rise and fall of the ions from one stratum to the other. On the other hand the horizontal and the vertical components are the only ones which the terrestrial ionic circuits between the two poles will affect, while the declination is wholly subordinate. The rotation of the Sun on its axis will in any event, whether the energy is transported in the oval polar circuit or as a linear radial radiation, not much influence the declination from day to day. It was a misapplication to assert that negative results of the periodogram carry with them a decisive critical meaning regarding any solar period. This tendency to mix up terrestrial and solar data in the same mass of numerical quantities, has been also found in the count of the number of the solar prominences, and is no doubt to some extent unavoidable since in our common observations we are not readily able to distinguish between them, but it is my opinion that in the present stage of the science it is better to employ a simple comparison of the data as they stand for the discovery of synchronism and periodicity, rather than to bury the several impulses in one massive computation. What is at present urgently required in this research, is to bring together all the data in simple homogeneous series, as in a carefully constructed ephemeris, for each of the several elements of the entire problem, determine what is solar and what terrestrial, and then introduce terms in the functions which will give some chance of separating the unknowns in a satisfactory analysis. This result will best be reached by intelligent coöperation, and I have no doubt that practical methods may be devised by means of which this purpose can be accomplished. It seems to me a very important advance to have gained a general view from which to correlate and harmonize so many of the problems that have heretofore been insoluble.

Studies on the Diurnal Periods in the Lower Strata of the Atmosphere.

MONTHLY WEATHER REVIEW.

THE VARIATION OF LATITUDE.

In August 1903 the appearance of the first volume of results of the Variation of Latitude campaign was signalized in this Magazine by a short history ot the whole subject up to the end of the year 1902. Of the second volume*, which has just appeared, there seems little to say here, as it consists mainly of tables of figures similar to those of the first volume, explanation of which may be found in the number above quoted. This second volume gives the results of the work during the years 1902, 1903, and 1904, which may be summarized by saying that in 1902 the pole of the Earth moved about its mean position in a curve approximating to a circle of 0″.15 radius. In 1903 the circle was somewhat larger, the distance of the pole from its mean position coming to a maximum of about 0″.30 in June of that year. In 1904 the spiral curve began to turn inwards and ended at the beginning of 1905, at a distance of 0″.15 from the mean position.

But between the appearance of these two volumes several minor memoirs on the subject have been published. An excellent summary of these by M. R. Radau appeared in the *Bulletin Astronomique* for May last, which we take leave to translate.

M. Albrecht published in two notes the provisional results of observations in the years 1903 and 1904, which have been discussed by MM. Wanach and Förster. In 1903 the amplitude of variation appeared to have reached its maximum, for in 1904 it decreased a little. The variation of latitude is represented by the formula:

$$x \cos l + y \sin l + z,$$

where the annual term z is included, independent of the longitude λ, which was discovered by M. Kimura. To obtain an idea of the nature of this term, the following table, which gives its value for every tenth of a year, may be examined:—

	1900	1901	1902	1903	1904.
	″	″	″	″	″
0 . .	+0.028	+0.062	+0.025	+0.036	+0.059
1 . .	+0.021	+0.055	+0.013	+0.046	+0.056
2 . .	+0.008	+0.022	−0.003	+0.029	+0.027
3 . .	−0.014	−0.005	−-0.024	−0.013	0.900
4 . .	−0.029	−0.026	−0.043	−0.012	−0.014
5 . .	−0.033	−0.036	−0.053	−0.029	−0.015
6 . .	−0.025	−0.032	−0.046	−0.022	−0.007
7 . .	−0.008	−0.016	−0.022	−0.005	+0.001
8 . .	+0.019	+0.007	+0.008	+0.022	+0.019
9 . .	+0.047	+0.025	+0.023	+0.043	+0.047

* 'Resulate des Internationalen Breitendienstes.' Band II. Von Th. Albrecht und B. Wanach. Berlin, 1906.

It can be seen that it is a question of hundredths of seconds, and it is instructive to note that the mean values of latitudes themselves vary from one year to another by quantities of the same order, as the following table shows:—

Mean Annual Values of Latitude (39° 8′ +).

	1900-01	1902	1903	1904
	″	″	″	″
Mizusawa	3.62	3.64	3.62	3.62
Tchardjui	10.67	10.66	10.69	10.66
Carloforte	8.93	8.93	8.91	8.94
Gaithersburg . .	13.20	13.21	13.18	13.14
Cincinnati	19.31	19.33	19.34	19.36
Ukiah	12.07	12.04	12.06	12.07

There are then discordances of 5 or 6 hundredths of a second between the annual means, and a term with an annual period of which the amplitude is often less than these discordances.

The Japan station of Mizusawa excels all the others in the quality of observations, judging from their theoretical weight. It may be added that the Observatory of Tokio furnishes each year a contingent of observations, which agree perfectly with those of Mizusawa. The Observatory of Leyden continues to note the variations of latitude.

From the beginning of 1903, M. Kimura has undertaken at Mizusawa, with his assistant M. Nakano, a more extended series of observations than the international scheme required. The discussion of this material showed that there existed no systematic difference between results drawn from the observations of two groups of stars observed each night and those deduced from four groups. A subsidiary result of the same series is that it furnished proof that there was no diurnal variation of latitude, or, at any rate, no appreciable variation (*Ast. Nach.* 4040-41).

In two previous notes (*Ast. Nach.* 3932, 3981) M. Kimura considered the period of the principal term of the variation, which is supposed to be about 14 months, and which he brought to 14½. He established, in fact, that the curves plainly indicate a period of six years, which ought to correspond to five periods of the principal term; the value of the period sought would then be 12 years = 438 days. A more precise consideration has shown that the cycle of six years is really 5.98, which gives for the period in question 436 or 437 days, instead of 428 or 430.

Professor H. G. van de Sande Bakhuyzen in a very interesting article (*Ast. Nach.* No. 3937) passes in review the hypotheses which have been put forward to explain the origin of the enigmatic term discovered by Kimura. It is known that Chandler has shown that this term can be explained if we admit a mean

stellar parallax of 0″.13, but this parallax appears to be much larger than one has a right to suppose. It is possible to invoke an annual oscillation of the center of gravity of the terrestrial globe? Adopting for the term in question (z) an amplitude of 0″.04, the periodic displacement of the center of gravity ought to have an amplitude of 1.5 meters; the total displacement would be three meters. This might be attributed to the melting of the polar ice, but it is necessary to suppose that the melting removes each year three million cubic kilometers of ice, that is a layer one kilometer thick, covering a polar cap of nine degrees radius. The heat that the Sun sends us would not be sufficient to produce this effect. It is true that one can find other causes for an oscillation of this kind. Such a one has been indicated by M. R. Schumann (*Ast. Nach.* 3877). It may also be asked if all these variations are real or if they are due, at least in part, to instrumental errors or to the influence of anomalies of refraction.

The observations which have been made at Leyden since 1899 with a Wanschaff zenith-telescope may be profitably examined to elucidate this question. Attempt has been made to determine the value of z for the three years 1900, 1901, and 1902. But the numbers that Professor Bakhuyzen has found at Leyden have little resemblance to those that Professor Albrecht gives for the whole of the international stations; the amplitudes and especially the phases are very different. But this discordance, since it happens in measures so delicate and which nearly touch the limits of observation, is in truth what might be expected. Perfect accordance would leave us sceptical.

Among the instrumental errors which cause apparent variation of latitude, M. Kimura has noted those which arise from the influence of temperature upon the micrometers (*Ast. Journal*, vol. xxiii). But the Leyden observations do not appear to show the existence of errors from this source.

There remain the anomalies of refraction caused by variable inclination of layers of equal density. This inclination is certainly subject to slight local variations according to the state of the atmosphere. There are also troubles which result from inequality of temperatures outside and inside the observing-room, as the researches of MM. Faye, Bakhuyzen, Nyrén and Schumann have shown, a question which was discussed in the Annual Report of the Geodetic Institute (1893-1894. p. 7). But how can it be supposed that these variations are the same for the six stations situated in climates so diverse? It seems, on the other

hand, that the judicious installation of these stations disposes of anomalies of this kind.

There is, we think, pressing need of undertaking methodical observations for the simultaneous study of astronomical refraction and of the state of the upper layers of the atmosphere, as Admiral Mouchez proposed to make at the Paris Observatory in 1882 by the aid of a captive balloon. The researches that eminent astronomers have recently effected to determine, in a more exact manner, the constants which enter into the theoretical expression for the refraction would have given us complete information on this question if care had previously been taken to find out the distribution of temperature and pressure in the plane of the meridian up to a certain height. It would have been sufficient for this to have set up registering apparatus.

THE OBSERVATORY, August 1906.

THE NEW PLANET TG.

A. C. D. CROMMELIN.

The zone of asteroids has certainly attracted more attention from astronomers since the discovery of Eros in 1898. There was a natural expectation that further sensational orbits might be discovered, and after a lapse of eight years this expectation has been realized. The planet TG is indeed of less practical importance than Eros, which has already proved so useful in measuring the Sun's distance, but its theoretical interest is nearly, if not quite, as high. The new planet is remarkable for its very large mean distance, which is practically identical with that of Jupiter; and as it has a considerable eccentricity, its aphelion lies far outside Jupiter's orbit, so that the family of asteroids, as now known, ranges from distance 1.1 (perihelion of Eros) to distance 6.1 (aphelion of TG). They have in fact burst their bounds in each direction, going far inside the orbit of Mars and far outside that of Jupiter. It is probable that the disturbing action of these two planets has brought Eros and TG into their present orbits. Herr Dziewulski, of Cracow, has recently published some researches on the motion of Eros, in which he takes account of the modification of its orbit produced by the action of the other planets, and finds that about B. C. 7,400 its orbit intersected that of Mars, so that very close approaches of the two planets would be likely to occur. It is, in fact, very probable that the orbit of Eros was deflected into its present position by such an

encounter or series of encounters with Mars. Its orbit even now cannot be looked on as perfectly stable, for about A.D. 8,800 it will again intersect that of Mars, and further notable changes are likely to occur, whose exact character it is at present impossible to determine.

It would at first sight appear that TG was in still greater danger of destructive changes in its orbit than Eros, for its troublesome neighbor is not the small and feeble Mars, but the giant planet Jupiter, notorious for the mighty disturbances which he produces in the orbits of heavenly bodies so unfortunate as to pass near him. However, Professor C. V. L. Charlier (in *Astr. Nachrichten*, No. 4,094) indicates a possible way of escape for TG from such destructive changes, which will at the same time afford a most interesting illustration of a principle which had already been recognized as theoretically possible, although no actual case was known. The "Problem of Three Bodies" or the attempt to arrive at expressions which will give their mutual positions at any time we may desire, is probably insoluble in the general case, though the results of lunar and planetary theory have been arrived at, thanks to the circumstance that in these cases one of the attracting bodies is greatly superior to the others in its action, and a solution by successive approximation is thus rendered possible. There are, however, a few simple cases where a rigorous solution of the problem has been arrived at. One is when the three bodies are in a straight line and also moving in that line. In this case elementary methods suffice for a solution (at least so long as no collision takes place; if such happens a knowledge of the elasticity of the bodies is required, before we can predict their subsequent behavior). A more interesting case is the one which Dr. Charlier considers may very probably be exemplified by the Sun, Jupiter and TG. Lagrange showed that if three bodies are placed at the angles of an equilateral triangle and projected with appropriate speeds in their proper directions, they will continue to form an equilateral triangle unless disturbed by some extraneous force such as a fourth body, a resisting medium, etc. Dr. Charlier himself continued the investigation in "Publications of the Lund Observatory," No. 18, and showed that the system might still be stable, even if the triangle were not accurately equilateral to start with; we should in this case get oscillatory motion, never deviating very widely from the symmetrical form. There are two reasons for thinking that the three bodies named above form a system of this kind: (1) The period found for TG by Dr. Berberich (viz., 12.02 years) is almost

exactly the same as that of Jupiter (11.86 years). As the planet has only been under observation for three months it is quite possible that Dr. Berberich's period is in error by this trifling amount (about two months), and that the periods are really exactly equal. We shall probably have to wait till next year before this point is certainly known. (2) The angle subtended at the Sun between Jupiter and TG does not differ greatly from 60°. It was in fact 55° 31′ on February 22 last, which is quite within the possible range of oscillation about a mean value of 60°.

Assuming that the period of TG is really 11.86 years, Dr. Charlier finds that its motion can be approximately pictured as follows: (1) Consider a point traveling in an ellipse about the Sun with the period 11.86 years, but with the perihelion point advancing so that the period from the perihelion passage to the next is 11.90 years. (2) Consider TG as moving in a small ellipse round this point in a period of 148 years. The size of the ellipse will be deducible when the elements of TG are better determined, but it may be large enough to produce a very noticeable shift in the position of TG. Dr. Charlier has considered the case of a small planet whose orbit plane coincides with that of Jupiter. The plane of TG deviates very notably from this, and there will, I suppose, be similar perturbations in latitude, viz., one with a period of about twelve years, which is practically the principal term in latitude, and another with a much longer period. Of course TG will be liable in addition to perturbations by the other planets, of which Saturn will play by far the greater *rôle*. These, however, will be of an oscillatory character, and will probably not tend to derange permanently the balance of the system. It is to be hoped that Dr. Charlier's expectations may be verified, and that TG may indeed prove to exemplify a type of motion so wonderfully anticipated by the great Lagrange more than a century ago. To strengthen his case he states as a conjecture that Jupiter would probably compel a planet that had a period nearly the same as his to take up this exact period, since otherwise its motion would seem to be unstable. He makes the suggestion that the regions 60° distant from Jupiter on each side should be diligently swept for other bodies of the same type, and even extends the suggestion to the case of Saturn, though the prospects here are not very encouraging.

TG must be a fairly large asteroid; judging from its brightness its diameter can scarcely fall below 100 miles, which far exceeds the estimates for most of the recent discoveries. Professor Barnard deduced that Ceres headed the list with 480 miles, Vesta,

although brighter, being only 240 miles in diameter. Dr. Berberich's elements follow; it will not be difficult with their aid to insert the orbit in a diagram of the Solar System.

Epoch 1906, Feb. 22. Berlin Midnight.		Period 12.02 years.	
Mean Longitude	48° 57′	Mean distance from Sun	5.248
Longitude of Perihelion	76° 0′	Least " "	4.369
" " Asc. Node	315° 34′	Greatest " "	6.127
Inclination to Ecliptic	10° 21′	—	
Eccentricity	0.1676	—	

KNOWLEDGE AND SCIENTIFIC NEWS. September, 1906.

THE MILKY WAY AND THE THEORY OF GASES.*

H. POINCARÉ.†

The matter of which I wish to speak to you today has up to this time attracted but little attention from astronomers; I could scarcely make note of anything except an ingenious idea of Lord Kelvin, which has opened a new field of research but which is still waiting for someone to enter upon it. Neither have I any original results of which to tell you, and all that I can do is to give you an idea of the problems that present themselves but which no one has as yet undertaken to solve.

You all know how a large number of modern physicists imagine the composition of gas; gases are formed by an innumerable multitude of molecules which, animated with great velocity, cross and intercross in all directions. These molecules probably act upon each other at a distance, but this action decreases very rapidly with the distance so that their trajectories remain apparently rectilinear; they do not cease to be so except when two molecules happen to pass quite near to each other; in this case their mutual attraction or repulsion makes them deviate to the right or to the left. This is what is sometimes called a shock; but there is no occasion to understand the word *shock* in its usual significance; it is not necessary that the two molecules come in contact: it suffices that they approach each other near enough to become sensible of their mutual attractions. The laws of deviation which they follow are the same as if there had been an actual shock.

It seems at first that the disordered shocks of this countless dust can only create an inextricable chaos before which the ana-

* Bulletin Astronomique de la Société Astronomique de France, April, 1906.

† M. Poincaré former President of the Astronomical Society of France, is now of the Institute.

lyst must draw back. But the law of large numbers, that supreme law of chance, comes to our aid; face to face with a semi-disorder we should have to despair, but in extreme disorder this static law re-establishes a sort of mean order to which the mind can adjust itself. It is the study of this mean order which constitutes the kinetic theory of gases; it shows us that the velocity of the molecules is equally distributed in all

FIGURE 1. THE MILKY WAY IN THE NORTHERN HEMISPHERE.

directions, that the magnitude of these velocities varies from one molecule to another, but that this variation even is subject to a law called Maxwell's law. This law tells us how many molecules there are animated with such and such velocity. As soon as the gas departs from this law, the mutual shocks of the molecules, by modifying the magnitude and the direction of their velocities, tends to bring them back promptly. The physicists have tried,

not without success, to explain in this manner the experimental properties of gases, for instance the law of Mariotte.

Let us now consider the Milky Way; there also we see countless dust, only the grains of this dust are no longer atoms, they are stars; these grains also move with great swiftness; they act upon each other at a distance, but this action is so small at a great distance that their trajectories are rectilinear; and yet, from time to time, two of them may approach each other enough to be swerved from their route, like a comet which has passed too near to Jupiter. In a word, to the eyes of a giant for whom our Suns would be what atoms are for us, the Milky Way would seem only a bubble of gas.

Such was the leading idea of Lord Kelvin. What can we conclude from this comparison? To what extent is it exact? That is what we shall try to find out together; but before reaching a definite conclusion and without wishing to prejudge it, we foresee that the kinetic theory of gases will be for the astronomer a model which he ought not to follow blindly, but with which he can usefully inspire himself. Up to the present, celestial mechanics has attacked only the solar system or a few double star systems. In face of this unity presented by the Milky Way, or the star clusters, or the resolvable nebulae, it drew back, because it saw there nothing but chaos. But the Milky Way is not more complicated than a gas; the static methods founded on a calculation of probabilities applicable to the one are also applicable to the other. First of all it is important to note the resemblance of the two cases and their difference.

Lord Kelvin strove to determine by this means the dimensions of the Milky Way; for that, one is reduced to counting the stars visible in our telescope; but we are not sure that, beyond the stars which we see there are not others which we do not see; so that what we should measure in this manner would be not the size of the Milky Way but the power of our instruments. The new theory will offer us other resources. We are indeed acquainted with the movements of the stars nearest to us and we form an idea of the greatness and the direction of their particular velocities. If the ideas expressed above are correct, these velocities should follow Maxwell's law, and their average value will let us know, so to speak, that which corresponds to the temperature of our imaginary gas. But this temperature itself depends on the dimensions of our gaseous bubble. In fact, how will a gaseous mass abandoned in space act, if its elements attract each other according to the law of Newton? It will

take the spherical form; moreover, on account of gravitation, its density will be greater in the center, the pressure also will increase from the surface to the center because of the weight of the exterior parts attracted toward the center; finally the temperature will increase toward the center: the temperature and the pressure being connected by the law called adiabatic, as happens in the successive layers of our atmosphere. At the very surface,

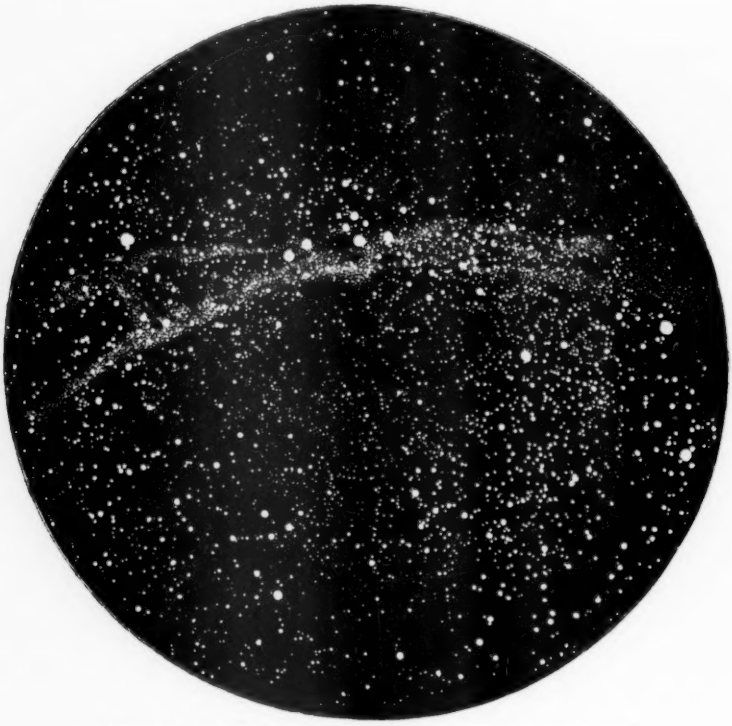


FIGURE 2. THE MILKY WAY IN THE SOUTHERN HEMISPHERE.

the pressure will be zero, and it will be the same with the absolute temperature, i.e. with the velocity of the molecules.

Here a question presents itself: I have spoken of the adiabatic law, but this law is not the same for all gases, since it depends on the relation of their two specific heats; for the air and analogous gases this relation is 1.42; but ought the Milky Way to be likened to the air? Evidently not; it should be considered as

a monatomic gas, like the vapor of mercury, like argon, like helium, that is to say, the relation of their specific heats should be considered equal to 1.66. In fact, one of our molecules would be, e.g., the solar system; but the planets are only very small personages, the Sun alone counts, so that our molecule is truly monatomic. Even if we take a double star, it is probable that the action of a strange star, which might happen to come near it, would become perceptible enough to deflect the general movement of transfer of the system long before being capable of disturbing the relative orbits of the two components; in a word, the double star would act like an indivisible atom. However that may be, the pressure and consequently the temperature at the center of the gaseous sphere would be correspondingly greater as the sphere was larger, since the pressure increases with the weight of all the overlying layers. We can suppose that we are nearly at the center of the Milky Way, and in observing the average velocity proper to the stars we shall learn what corresponds to the central temperature of our gaseous sphere and we shall determine its radius.

We can form some idea of the result from the following considerations. Let us take a simpler hypothesis: the Milky Way is spherical and its masses are distributed in a homogeneous manner; the result is that the stars within it describe ellipses having the same center. If we suppose that the velocity becomes zero at the surface, we can calculate this velocity at the center, by the equation of living forces. We thus find that this velocity is proportional to the radius of the sphere and to the square root of its density. If the mass of this sphere were that of the Sun and its radius that of the terrestrial orbit, it is easy to see that this velocity would be that of the Earth in its orbit. In the case that we have supposed, the mass of the Sun should be distributed in a sphere with a radius one million times larger, this radius being the distance of the nearest stars; the density is then 10^{18} times less; now the velocities are of the same order, hence it must be that the radius is 10^{9} times greater, that is one thousand times the distance of the nearest stars, which would make about one thousand millions of stars in the Milky Way.

You will say that these hypotheses diverge widely from reality; in the first place the Milky Way is not spherical and we shall soon return to this point; and then the kinetic theory of gases is not compatible with the hypothesis of a homogeneous sphere. But in making the exact calculation according to this theory one would find no doubt a different result but one of the same order;

now in such a problem the data are so uncertain that the order of dimension is the only end at which we are to aim.

Here a first remark presents itself; the result of Lord Kelvin which I have just verified by an approximate calculation obviously agrees with the estimates which the observers have been able to make with their telescopes; so that we must conclude that we have very nearly pierced the Milky Way. That

FIGURE 3. THE GREAT ANDROMEDA NEBULA.
Photographed by H. C. Wilson.

enables us to decide another question. There are the stars which we see because they shine; but might there not be obscure stars which circulate in the interstellar spaces and whose existence might long remain unknown? Very well then, that which Lord Kelvin's method would give us would be the total number of stars including the dark ones; since his number is comparable

to that which the telescope gives, then there is no dark matter, or at least not so much as there is of shining matter.

Before going further we ought to look at the problem from another side. Is the Milky Way constituted of a gas, properly so-called? It is known that Crookes introduced the notion of a fourth state of matter in which gases having become too rarefied are no longer true gases and they become what is called radiant matter. The Milky Way—considering the smallness of its density, will it be like gaseous matter or like radiant matter? It is the consideration of what is called the *libre parcours* (free course) which will furnish us the answer.

The trajectory of a gaseous molecule may be regarded as formed of rectilinear segments joined by very small arcs corresponding to successive shocks. The length of each one of these segments is what is called the free course; this length of course, is not the same for all the segments and for all the molecules; but we can take an average; that is what is called the mean course (*parcours moyen*); this increases as the density of the gas diminishes. The matter will be radiant if the average course is greater than the dimensions of the vessel where the gas is enclosed, so that a molecule may chance to travel through the entire vessel without suffering any shock; in the other case it remains gaseous. From this it results that the same fluid may be radiant in a small vessel and gaseous in a large vessel; it is perhaps on account of this that, in one of Crookes' tubes, the larger the tube is the farther one must force the vacuum.

What happens, then, for the Milky Way? It is a mass of gas whose density is very low but whose dimensions are very great; does a star have chances to cross it without suffering a shock, i.e. without passing near enough to another star to be perceptibly swerved from its course? What do we understand by *near enough*? That is necessarily a little arbitrary; let us say that it is the distance from the Sun to Neptune, which would represent a deviation of about ten degrees; then let us suppose each one of our stars enveloped in a protecting sphere of this radius; will a straight line be able to pass between these spheres? At the average distance of the stars of the Milky Way the radius of one of these spheres will be seen at an angle of about one-tenth of a second; and we have a thousand million stars! Let us place on the celestial sphere a thousand million little circles with a radius of a tenth of a second. Is there any chance that these circles will cover the celestial sphere a great number of times? Far from it; they will only cover the sixteen-thousandth part. Thus the Milky

Way is not the image of gaseous matter but of the radiant matter of Crookes. Nevertheless, since our preceding conclusions are fortunately not very precise, we do not have to modify them perceptibly.

There is another difficulty; the Milky Way is not spherical, and up to this time we have reasoned as though it were, since that is the form of equilibrium which a gas isolated in space would take. In support of this theory there exist star clusters whose form is globular and to which what has just been said applies better. Herschel had already endeavored to explain their remarkable appearance. He supposed that the stars of the clusters

FIGURE 4. TYPE OF GLOBULAR CLUSTER: CLUSTER IN HERCULES.
Photographed by Isaac Roberts.

are uniformly distributed in such a manner that one cluster would be a homogeneous sphere; each star then would describe an ellipse and all these orbits would be traveled over in the same time, so that at the end of a period the cluster would find again its primitive configuration and that this form would be stable. Unfortunately the clusters do not appear homogeneous; one observes condensation at the center, one would observe it even if the sphere were homogeneous, since it is thicker at the center; but it would not be so marked. We can then rather compare a cluster

to a gas in adiabatic equilibrium and which takes the spherical form because that is the form of equilibrium of a gaseous mass.

But, you will say, these clusters are much smaller than the Milky Way of which they probably even form a part, and although they are denser they will still give us something more analogous to radiant matter; now gases reach their adiabatic equilibrium only by a succession of innumerable shocks of the molecules. There might be some way of arranging that. Let us suppose that the stars of the cluster have just enough energy so that their velocity is annulled when they reach the surface; then they will be able to pass through the mass without shock, but, having reached the surface they will turn backward and cross it again; after a great number of crossings they will end by being turned aside by a shock; in these conditions we should still have a matter that could be considered as gaseous; if by chance there had been in the cluster stars whose velocity was greater, they have left it long ago, they have gone away to return there no more. For all these reasons it would be interesting to examine the known clusters, to try to explain the law of their densities and to see if it is the adiabatic law of gases.

But let us return to the Milky Way; it is not spherical, but is rather to be described as a flattened disk. It is plain then, that a body leaving the surface without velocity will arrive at the center with different velocities, according as it leaves the surface in the neighborhood of the middle of the disk or at its edge; the velocity would be considerably greater in the latter case.

Now up to this point we have admitted that the real velocities of the stars, those which we observe, should be comparable to those which similar masses would attain; this leads to some difficulty. We have given above a value for the dimensions of the Milky Way, and we have deduced it from the observed real velocities which are of the same order of magnitude as that of the Earth in its orbit; but what is the dimension that we have thus measured? Is it the thickness? Is it the radius of the disk? It is doubtless something intermediate; but what can we say then of the thickness itself or of the radius of the disk? Data fail me for making this calculation; I limit myself to giving you a glimpse of the possibility of basing a valuation at least approximate on a thorough discussion of the proper motions.

We find ourselves then in the presence of two hypotheses: either the stars of the Milky Way are animated with velocities which are for the most part parallel to this galactic plane, but otherwise distributed uniformly in all directions parallel to this plane. If it

is so, the observing of the proper motions ought to reveal to us a preponderance of components parallel to the Milky Way; this is yet to be determined, for I do not know whether a systematic discussion has been made from this point of view. On the other hand, such an equilibrium could be only temporary, for in consequence of the shocks, the molecules, that is to say the stars, will in time reach remarkable velocities in the direction perpendicular to the Milky Way and they will end by quitting its plane, so that the system will tend toward the spherical form, the only form of equilibrium of an isolated gaseous mass. Or else, the entire system is animated by a common rotation, and it is for this reason that it is flattened like the Earth, like Jupiter, like all bodies which revolve. Only, as the flattening is considerable it must be that the rotation is rapid; rapid indeed, but we must agree on the meaning of this word. The density of the Milky Way is 10^{25} times less than that of the Sun; a velocity of rotation which is $\sqrt{10^{25}}$ times less than that of the Sun would then be its equivalent from the point of view of the flattening; a velocity 10^{12} times slower than that of the Earth, that is a thirtieth of a second of an arc in a century, would be a very rapid rotation, almost too rapid for stable equilibrium to be possible.

In this hypothesis the observable proper motions will appear to us uniformly distributed and there will no longer be any preponderance for the components parallel to the galactic plane. They will tell us nothing about the rotation itself since we are a part of the revolving system. If the spiral nebulae are other Milky Ways, strangers to our own, they are not drawn into this rotation and their own motions could be studied. It is true that they are very distant; if a nebula has the dimensions of the Milky Way and if its apparent radius is e.g. 20″, its distance is 10,000 times the radius of the Milky Way. But that will not matter, since it is not about the translation of our system that we are seeking information but about its rotation. The fixed stars by their apparent motion, plainly reveal to us the diurnal rotation of the Earth although their distance is immense. Unfortunately the possible rotation of the Milky Way, however swift it may be relatively, is very slow from the absolute point of view, and besides the pointings on the nebulae can not be made very exact; so that thousands of years of observations would be necessary in order to learn anything.

However that may be, in this second hypothesis the figure of the Milky Way would be a figure of definitive equilibrium. I

will no longer discuss the relative value of these two hypotheses for there is a third which is perhaps more probable. We know that among the irresolvable nebulae several families can be distinguished; the irregular nebulae, like that of Orion, the planetary and ring nebulae, the spiral nebulae. The spectra of the first two families have been determined, they are discontinuous; these nebulae are then not formed of stars; moreover, their distribution over the heavens seems to depend upon the Milky Way, whether they have a tendency to go away from it, or on the other hand to approach it, they form a part of the system. On the contrary the spiral nebulae are generally considered as independent of the Milky Way; it is admitted that they are, like it formed of a mul-

FIGURE 5. THE SPIRAL NEBULA OF CANUM VENATICORUM.
Photographed by Isaac Roberts.

titude of stars, that they are, in a word, other Milky Ways very distant from our own. The recent works of Stratonoff tend to make us consider the Milky Way itself as a spiral nebula, and that is the third hypothesis of which I wished to speak to you.

How shall we explain the very singular appearances presented by the spiral nebulae and which are too regular and too constant to be due to chance?

In the first place a glance at one of these pictures suffices to show that the mass is in rotation; even the direction of the rota-

tion, can be seen; all the spiral radii are curved in the the same direction; it is evident that it is the *advancing wing* which lags on the pivot and that determines the direction of rotation. But that is not all; it is clear that these nebulae can not be likened to a gas in repose, nor even to a gas in relative equilibrium under the influence of a uniform rotation; they must be compared to a gas in permanent motion in which internal currents rule.

Let us suppose, for example that the rotation of the central nucleus be rapid (you know what I mean by that word), too rapid for stable equilibrium; then at the equator the centrifugal force will surpass the attraction, and the stars will have a tendency to escape along the equator and will form divergent currents; but in receding, since their moment of rotation remains constant and the radius vector increases, their angular velocity will diminish, and that is why the advancing wing seems to lag behind. In this way of looking at it there would be no real permanent motion, the central nucleus would constantly lose matter which would go away to return no more and would gradually exhaust itself. But we can modify the hypothesis. In proportion as it recedes the star loses its velocity and finally stops; at this moment attraction seizes it again and brings it back toward the nucleus; there would then be centripetal currents. It must be admitted that centripetal currents are of first rank and centrifugal currents of second rank, if we take up again the comparison with a troop in battle executing a wheel; and in fact the component centrifugal force must be compensated by the attraction exercised by the central layers of the swarm upon the extreme outer layers.

Moreover, at the end of a certain time, a permanent order is established; the mass having curved itself, the attraction exercised on the pivot by the advancing wing tends to delay the pivot and that of the pivot on the advancing wing tends to accelerate the advance of this wing which no longer increases its lagging, so that finally all the radii end by turning with a uniform velocity. It may be admitted nevertheless that the rotation of the nucleus is more rapid than that of the radii.

There is one question; why do these centripetal and centrifugal swarms tend to concentrate themselves in radii instead of scattering themselves in all directions? Why do these radii divide regularly? If the swarms concentrate themselves, it is because of the attraction exercised by the swarms already existing upon the stars which come out of the nucleus, in their neighborhood. As soon as an inequality is produced it tends to accentuate itself on

this account.

Why do the radii divide regularly? This is a more delicate question. Suppose there is no rotation, that all the stars are in two rectangular planes in such manner that their distribution is symmetrical with relation to these two planes. In such symmetry, there would be no reason for their leaving these planes nor for the symmetry changing. This configuration would then give us equilibrium but *it would be an unstable equilibrium.*

On the contrary, if there is rotation we shall find a configuration of analogous equilibrium with four bent radii equal to each other and cutting each other at 90°, and if the rotation is rapid enough this equilibrium may be stable.

I am not prepared to state more exactly: it is enough for me to let you see that these spiral forms may perhaps be explained some day by simply calling in the law of gravitation and static considerations recalling those of the theory of gases.

What I have just said to you of internal currents shows you that there may be some interest in discussing systematically all available proper motions; that can be undertaken in a hundred years, when the second edition of the chart of the heavens will be compared with the first which we are now making.

But in closing I should like to call your attention to one question, that of the age of the Milky Way or of the nebulae. If what we believe we have seen should be confirmed, we can form some idea about this. This kind of static equilibrium of which gases give us the model can establish itself only after a great number of shocks. If these shocks are rare it can be produced only after a very long time; if the Milky Way (or at least the clusters which are a part of it), if the nebulae have really attained the equilibrium, it is because they are very old, and we shall have an inferior limit of their age. We would also have a superior limit; this equilibrium is not definitive and can not last forever.

Our spiral nebulae would be comparable to gases animated with permanent motions; but gases in motion are viscous and their velocities are finally exhausted. What corresponds here to viscosity (and which depends on the chances of shock to the molecules) is exceedingly weak, so that the present system may endure for an extremely long time, yet not always; so that our Milky Ways can not exist eternally nor become infinitely old.

And that is not all. Let us consider our atmosphere: at its surface must reign an infinitely small temperature and the velocity of the molecules is nearly zero. But it is only a question of mean velocity; in consequence of the shocks one of these molecules may

gain an enormous velocity (rarely it is true) and then it will depart from the atmosphere, and once gone it will never enter it again; our atmosphere is thus exhausting itself with extreme slowness. The Milky Way too will from time to time lose a star by the same mechanism and that also limits its duration.

Very well then, it is certain that if we compute in this manner the age of the Milky Way we shall find some enormous figures. But here is another difficulty. Certain physicists, relying upon other considerations, estimate that the suns can have only an ephemeral existence, about fifty millions of years; our minimum would be greater than that by far. Must we believe that the evolution of the Milky Way began when matter was still dark? But how did the stars which compose it reach the adult age all at the same time, an age which is to last so brief a space? Or are they rather to reach it successively and are those that we see only a small minority compared with those which are extinct or which will one day shine out? But how shall we reconcile that with what we have said above about the absence of dark matter in considerable proportion? Should we abandon one of the two hypotheses, and which one? I content myself with pointing out the difficulty without pretending to solve it; I will close then with a great interrogation point. The more so as it is interesting to state problems even when the solution of them seems very far distant.

[Translated by Miss Isabella Watson, Northfield, Minn.]

OBSERVATIONS OF RED STARS FOR COLOR EFFECT IN PARALLAX WORK.

In the TRANSACTIONS OF THE ASTRONOMICAL OBSERVATORY OF YALE UNIVERSITY, Vol. 2 pt. 1., is found on page 184, an article showing the effect of the red color of some stars on the observations of those stars for parallax.

We give below a large part of that article that our readers may see the relation between theory and observation in regard to this interesting question.

"The work of the preceding pages, in which every precaution to eliminate known sources of error was employed, appears to us to be free from all systematic error, except perhaps one due to the star's color. Any perceptible difference in the mean refrangibility of the light of two stars might possibly produce an effect upon the measured distance between them which would be a function of the hour angle, and hence affect their apparent rela-

tive parallax, since it is generally impossible for practical reasons with our instrument to make observations at the two different parallax epochs otherwise than on opposite sides of the meridian. To ascertain whether an error due to this cause is appreciable in actual observation Dr. Chase, in 1898 and 1899, made a series of observations on five highly colored stars selected from Krüger's *Catalog der Farbigen Sterne*, carrying out the investigation in the following manner:—

The plan was at the epoch when the star culminates about midnight to measure the distances between the red star and each of two nearly equally distant comparison stars, one preceding and the other following, and as nearly as possible on the same parallel of declination, at rather large hour angles both east and west of the meridian. By taking two comparison stars we were able, as in the parallax work, to correct for any change in the scale value, and to eliminate any errors varying with the time.

The refraction of two stars with light of a different mean refrangibility being represented by

$$\beta \tan z, \text{ and } (\beta + \Delta\beta) \tan z$$

the measured distance between two such stars should receive, besides the correction for differential refraction, the additional correction

$$\Delta\beta \tan z \cos (p - q)$$

where z denotes the zenith distance, p the position angle of the red star from the comparison star, q the parallactic angle, and $\Delta\beta$ the supposed color effect."

Omitting the six pages of tables we quote again—

"The following pages give the measures corrected for refraction and aberration, the value of tan z cos $(p - q)$ for each observed distance, and the sums, differences, corrections for temporary scale value, and corrected differences, as in the series for parallax determination. These last, by their variation from an assumed mean difference, furnish the equations of condition which follow, in which x and x' represent the required corrections to the assumed mean difference for each epoch, $\Delta\beta$ the quantity to be determined, whose coefficient is the difference of the values of tan z cos $(p-q)$ for the two distances, and the second member the observed difference for each night minus the assumed mean difference. The star Krüger 1181 was specially selected because it had a close neighboring star presumably of average color, and as a test of the value of the method this latter also was observed with respect to the same comparison stars in conjunction with the red star and symmetrically with it, so that the conditions

should be absolutely the same. For both stars, likewise, the treatment and solution were carried out in precisely the same manner.

If we arrange the results of the previous pages in the order of the stars' redness as given by Krüger we have—

			″ ″	
Krüger	985	Color 6.0	$\Delta\beta = -0.019 \pm 0.019$	Wt. 63.6
	1080	7.0	$+0.005 \pm 0.020$	64.4
	1078	7.1	$+0.009 \pm 0.015$	16.0
	1181	7.8	$+0.014 \pm 0.018$	55.7
	1108	8.7	$+0.046 \pm 0.017$	45.2
W. B. 15^h,	745	...	-0.003 ± 0.021	55.6

An inspection of these results would seem to indicate that there is a discernible color effect conforming with theory, the mean light of the red star being apparently refracted less than that of the comparison stars, except in the case of Krüger 985, although the amount is very small except for very red stars. This conclusion is corroborated by a comparison of the results for Krüger 1181 and W. B. 15^h, 745, investigated under exactly the same conditions. The result for the latter coming out so nearly zero is a most satisfactory confirmation of the accuracy of the work. That the color effect is not greater rather tends to confirm Sir David Gill's opinion that the heliometer observers' tendency is to bring the similarly colored parts of the stars' spectra rather than the brightest parts into coincidence.

Now what influence this possible source of error, if real, may have upon our parallax results can be easily shown. Scarcely

Decl.	q	$\tan z \cos(p-q)$.		$\frac{E-W}{3.50}\Delta\beta$
		East	West	
°	°			
0	30.3	−0.59	+0.59	$-0.34\,\Delta\beta$
+20	52.3	−0.94	+0.94	−0.54
+40	65.0	−1.08	+1.08	−0.62
+60	74.4	−1.14	+1.14	−0.65
+80	77.5	−1.15	+1.15	−0.66

any of the measures were made at a zenith distance greater than 60° and the average was about 50°. The table herewith gives the value of the factor $\tan z \cos(p-q)$ for each twenty degrees of declination at 50° zenith distance, assuming p, the position angle of the red star from the comparison star to be 90°, for which value the difference of the factors for east and west observations is found to be a maximum. And taking the average double parallax factor to be 3.50 the last column gives the ex-

treme correction that would have to be applied to the parallax, in which for $\Delta\beta$ is to be taken its value for a star of the same degree of redness.

Thus considering that the color effect for a most highly colored star is not likely to be greater than $0.66 \times 0''.046 = 0''.03$ we feel justified in concluding that any vitiation in our parallax results, due to this cause, is presumably well within their probable error."

PLANET NOTES FOR NOVEMBER, 1906.

H. C. WILSON.

THE CONSTELLATIONS AT 9:00 P. M., NOVEMBER 1, 1906.

Mercury will be at greatest elongation, east from the Sun 23° 0′, on November 9 and so will be visible as evening star during the first half of the month. The planet will be at inferior conjunction November 29.

Venus also will be at inferior conjunction November 29 and the two planets Venus and Mercury will be in conjunction with each other first on November 14 and again on November 30. On the earlier of the two dates it may be possible to see the two planets together, just after sunset, with the aid of field-glasses. Mercury will then be about 2° north of Venus.

Mars is coming out of the morning twilight and will rise about three hours before the Sun in the early days of November. The disk of Mars is so small now, being only a little over 4″ in diameter, that little can be made out of the surface markings. Mars is to be found in the constellation Virgo northwest from the star Spica.

Jupiter is in good position for study in the morning hours. The planet is in the constellation Gemini and about as far north in declination as it ever gets. Jupiter is almost stationary for the first days of November but will move slowly westward during the month.

Saturn will reach quadrature, 90° east from the Sun, at the end of the month, and so is in good position for study during the early evening hours. Saturn is in the constellation Aquarius with no bright star in the vicinity.

Uranus is getting too near the Sun for observation.

Neptune is to be found in Gemini about 2° southeast from Jupiter, on November 1. Its motion this month will be slowly westward.

Occultations visible at Washington.

Date 1906.	Star's Name	Magnitude.	IMMERSION. Washington M. T.	Angle from N pt.	EMERSION. Washington M. T.	Angle from N pt.	Duration.
			h m	°	h m	°	h m
Nov. 2	63 Tauri	5.7	20 05	70	20 59	278	0 54
3	*m* Tauri	5.0	17 13	103	18 26	248	1 13
4	B.D.+19,°1110	6.0	13 13	96	14 42	243	1 29
11	*b* Virginis	5.2	18 47	152	19 53	266	1 06
18	14 Sagittarii	5.6	6 02	167	6 13	186	0 11
21	31 Capricorni	6.3	5 43	81	7 01	237	1 18
21	*c* Capricorni	4.3	8 23	44	9 24	273	1 01
22	39 Aquarii	6.2	5 09	86	6 29	226	1 20
22	45 Aquarii	6.1	9 42	62	10 47	250	1 05
24	Mayer 1012	6.3	2 11	91	3 12	229	1 01

Phases of the Moon.

Central Standard Time.

		h	m
Last Quarter	Nov. 8	15	44
New Moon	15	14	36
First Quarter	22	6	39
Full Moon	30	5	07

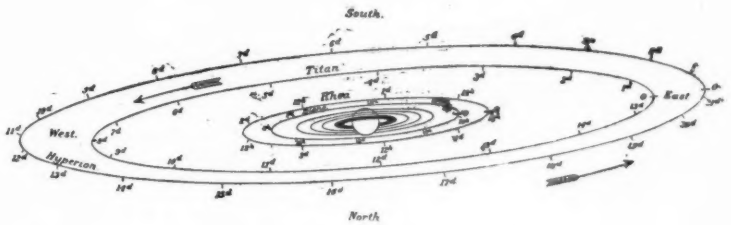
The Satellites of Saturn.

Apparent orbits of the seven inner satellites of Saturn as seen in an inverting telescope.

Central Standard Time reckoning from noon.

I. Mimas. Period 0^d $22.^h6$.

	h		h		h		h
Nov. 4	10.6 W	Nov. 12	10.9 E	Nov. 21	9.8 W	Nov. 29	10.1 E
5	9.2 W	13	9.5 E	22	8.4 W	30	8.7 E
6	7.9 W	14	8.1 E	23	7.0 W		
7	6.5 W	15	6.7 E	24	5.6 W		
8	5.1 W	16	5.4 E	25	4.3 W		



II Enceladus. Period 1^d $8.^h9$.

Nov.	2	6.3	E	Nov.	9	2.7	E	Nov.	15	23.2	E
	3	15.2	E		10	11.6	E		17	8.1	E
	5	0.1	E		11	20.5	E		18	17.0	E
	6	9.0	E		13	5.4	E		20	1.8	E
	7	17.8	E		14	14.3	E		21	10.7	E

Nov.	22	19.6	E
	24	4.5	E
	25	13.4	E
	26	22.3	E
	28	7.2	E
	29	16.1	E

III Tethys. Period 1^d $21.^h3$

Nov.	2	18.3	E	Nov.	10	7.5	E	Nov.	17	20.8	E
	4	15.6	E		12	4.8	E		19	18.1	E
	6	12.9	E		14	2.1	E		21	15.4	E
	8	10.2	E		15	23.5	E		23	12.7	E

Nov.	25	10.0	E
	27	7.4	E
	29	4.7	E

IV Dione. Period 2^d $17.^h7$.

Nov.	2	16.4	E	Nov.	10	21.5	E	Nov.	19	2.5	E
	5	10.1	E		13	15.2	E		21	20.2	E
	8	3.8	E		16	8.9	E		24	13.9	E

Nov.	27	7.7	E
	30	1.4	E

V Rhea. Period 4^d $12.^h5$.

Nov.	2	14.1	E	Nov.	11	14.9	E	Nov.	20	15.8	E
	7	2.5	E		16	3.4	E		25	4.3	E

Nov.	29	16.7	E

VI Titan. Period 15^d $23.^h3$.

Nov.	3	10.2	I	Nov.	11	3.5	S	Nov.	19	8.8	I
	7	7.8	W		15	5.2	E		23	6.6	W

Nov.	27	2.4	S

VII Hyperion. Period 21^d $7.^h6$.

Nov.		2.9	S	Nov.		14.7	I	Nov.		24.1	S
		8.5	E			19.7	W				

Nov.		29.7	E

VIII Iapetus. Period 79^d $22.^h1$.

Oct.		15.0	I	Nov.		4.2	W	Nov.		24.0	S

Dec.		13.4	W

Phenomena of the Satellites of Jupiter.

Central Standard Time, beginning at noon.

Nov.	1	14	48	I Sh. In.	Nov.	4	9	48	I Oc. Re.
		14	57	I Tr. In.			16	21	II Ec. Dis.
		17	04	I Sh. Eg.		6	10	23	II Sh. In.
		18	14	I Tr. Eg.			12	40	II Tr. In.
	2	11	58	I Ec. Re.			13	17	II Sh. Eg.
		15	21	I Oc. Re.			15	30	II Tr. Eg.
	3	9	16	I Sh. In.		7	9	42	III Ec. Re.
		10	24	I Tr. In.			11	14	III Oc. Dis.
		11	32	I Sh. Eg.			14	14	III Oc. Re.
		12	41	I Tr. Eg.		8	10	34	II Oc. Re.
		17	9	III Sh. In.			16	41	I Sh. In.

Phenomena of the Satellites of Jupiter.—(Continued.)

Nov.	8	17	45	I Tr. In.	Nov. 19	9	49	I Sh. Eg.
	9	13	51	I Ec. Dis.		10	42	I Tr. Eg.
		17	09	I Oc. Re.	20	7	49	I Oc. Re.
	10	11	10	I Sh. In.		15	39	II Sh. In.
		12	12	I Tr. In.		17	23	II Tr. In.
		13	26	I Sh. Eg.	21	14	57	III Ec. Dis.
		14	29	I Tr. Eg.		17	42	III Ec. Re.
	11	8	20	I Ec. Dis.	22	10	49	II Ec. Dis.
		11	36	I Oc. Re.		15	16	II Oc. Re.
	12	7	55	I Sh. Eg.	24	7	45	II Sh. Eg.
		8	56	I Tr. Eg.		9	22	II Tr. Eg.
	13	13	03	II Sh. In.		14	47	I Sh. In.
		15	02	II Tr. In.		15	45	I Tr. In.
		15	52	II Sh. Eg.		17	14	I Sh. Eg.
	14	10	58	III Ec. Dis.	25	8	09	III Sh. Eg.
		13	42	III Ec. Re.		8	11	III Tr. In.
		14	47	III Oc. Dis.		11	12	III Tr. Eg.
	15	8	14	II Ec. Dis.		12	08	I Ec. Dis.
		11	56	II Oc. Re.		15	09	I Oc. Re.
	16	15	45	I Ec. Dis.	26	9	26	I Sh. In.
	17	13	04	I Sh. In.		10	11	I Tr. In.
		13	59	I Tr. In.		11	42	I Sh. Eg.
		15	20	I Sh. Eg.		12	28	I Tr. Eg.
		16	16	I Tr. Eg.		14	58	IV Sh. In.
	18	10	14	I Ec. Dis.		15	32	IV Sh. Eg.
		13	23	I Oc. Re.	27	6	36	I Ec. Dis.
		13	43	IV Oc. Dis.		9	35	I Oc. Re.
		15	00	IV Oc. Re.	28	6	54	I Tr. Eg.
	19	7	32	I Sh. In.	29	13	24	II Ec. Dis.
		8	25	I Tr. In.		17	33	II Oc. Re.

Note.—In., denotes ingress; Eg., egress; Dis., disappearance; Ec., eclipse; Oc., occultation; Tr., transit of the satellite; Sh., transit of the shadow.

COMET NOTES.

New Comet *e* 1906 (Kopff).—A new comet was discovered by Kopff of Heidelberg upon a photograph taken on the night of August 22. Later its image was found also on a photograph taken at the same place on August 20. It is a very faint object barely visible in a four or five inch telescope and difficult to observe with a large telescope. It is about 2′ in diameter with only a slight central condensation and no definite nucleus.

When discovered the comet was in the constellation Pegasus almost due east from the star ζ and between 3° and 4° distant. On October 1, it will be between 3° and 4° south, and about half as far west, from the same star. The motion is remarkably slow for a comet. The elements which have come to hand at this date (Sept. 20) are exceedingly discrepant, depending upon the very short apparent arc described by the comet in three or five days.

Those telegraphed earliest are by Crawford depending upon observations on August 24, 25 and 26. They do not represent later observations quite so well as those given by M. Ebell in A.N. 4118 depending upon observations of August 23, 25 and 27.

PRELIMINARY ELEMENTS OF COMET *e* 1906.

	Crawford	Ebell
T =	1907 Apr. 12.26 Gr. M. T.	1906 May 14.688 Berlin.
ω =	221° 38′	353° 32′.4
Ω =	230 02	279 37 .9
i =	12 45	9 45 .9
q =	1.118	1.398

Ephemeris of Comet *e* 1906.

[By M. Ebell, from ASTR. NACH. 4118].

1906		α			δ		log *r*	log Δ	Br.
		h	m	s	°	′			
Aug.	27	22	45	53	+10	10.2	0.3069	0.0220	0.91
	29		44	21	10	2.8			
	31		42	51	9	54.7	0.3150	0.0350	0.83
Sept.	2		41	23	9	45.7			
	4		39	58	9	36.0	0.3230	0.0490	0.75
	6		38	37	9	25.8			
	8		37	19	9	14.9	0.3310	0.0639	0.67
	10		36	6	9	3.7			
	12		34	57	8	52.0	0.3389	0.0797	0.60
	14		33	53	8	40.1			
	16		32	55	8	28.1	0.3468	0.0961	0.54
	18		32	2	8	16.0			
	20		31	14	8	3.8	0.3345	0.1132	0.48
	22		30	32	7	51.7			
	24		29	56	7	39.7	0.3621	0.1308	0.43
	26		29	26	7	27.9			
	28		29	1	7	16.4	0.3696	0.1487	0.38

Holmes' Periodic Comet (1906 *f*).—This comet was discovered at its third apparition, by means of a photograph taken at Heidelberg, August 28, $13^h\ 52^m.1$ Königstuhl mean time. The position was then R. A. $4^h\ 27^m\ 45^s$, Decl. +42° 28′. The magnitude is estimated at 15.5. The correction to Dr. Zwier's ephemeris is -6^s, −2′. The following portion of the ephemeris will serve to indicate the position of the comet during October.

EPHEMERIS OF HOLMES' COMET.

[From A.N. 4085.]

Gr. Midn.		R.A.			Decl.			Brightness
		h	m	s	°	′	″	
Oct.	2	4	34	16.6	+48	38	34	0.037
	4		34	38.1	+48	57	31	
	6		34	50.2	+49	16	04	
	8		34	52 6	+49	34	10	0.037
	10		34	45.2	+49	51	46	
	12		34	27.9	+50	08	51	
	14		34	00.6	+50	25	21	
	16		33	23.1	+50	41	13	0.038
	18		32	35.4	+50	56	24	
	20		31	37.8	+51	10	49	
	22		30	30.2	+51	24	26	
	24		29	12.9	+51	37	11	0.038
	26		27	46.2	+51	49	01	
	28		26	10.3	+51	59	52	
	30		24	25.8	+52	09	41	
Nov.	1		22	32.9	+52	18	25	0.038
	3		20	32.2	+52	26	01	
	5		18	24.3	+52	32	25	
	7		16	09.7	+52	37	35	
	9		13	49.3	+52	41	29	0.038
	11		11	23.7	+52	44	04	
	13	4	08	53.8	+52	45	18	

Elliptical Elements of Comet *e* 1906 Kopff.—A telegram has been received at this Observatory from Professor A. O. Leuschner at Berkeley, Cal., stating that the following elliptical elements and ephemeris of Kopff's comet

have been computed by Messrs. Crawford and Champreux, from observations made August 24, September 5, and September 15.

Time of perihelion passage	(T) May 2.09 G. M. T.
Perihelion minus node	(ω) 19° 29′
Longitude of node	(Ω) 263° 45′
Inclination	(i) 8° 44′
Perihelion distance	(q) 1.699
Eccentricity	(e) 0.5204
Period	(P) 6.67 years.

Sept. 23.5	R.A. 22h 28m 47s	Dec. + 7° 32′	Light 0.65
27.5	22 27 30	7 04	
Oct. 1.5	22 26 38	6 36	
5.5	22 26 13	6 10	" 0.51

Harvard College Observatory,
Cambridge, Mass. Sept. 21, 1906.
Astronomical Bulletin, No. 254.

VARIABLE STARS.

Variable Star 6.1904 Cephei.—In *A.N.* 4108 Mr. S. Blajko of the Moscow Observatory states that this variable is of the Algol type with a period of thirty-two days. It has two minima, falling to magnitude 9.14 at the principal minimum and to 8.93 at the secondary. The ordinary magnitude of the star is 8.58. The change of brightness at the principal minimum lasts about six days and the light curve is quite symmetrical. The secondary minimum does not recur at a constant interval and the observations are as yet insufficient to determine its formula. That for the principal minimum is

J.D. 2416250.9 + 32d.315 E, Gr. M. T.

The star is BD + 67°244 and its position for 1855 is R.A. 2h 54m 51s, Decl. + 67° 0′.6. From the Christiana A.G. Zones its place reduced to 1900.0 is R.A. 2h 58m 49s, Decl. + 67° 11′.4.

Variable 14.1904 Cygni.—In *A.N.* 4108 Mr. S. Blajko also gives elements of this star and classifies it as of the Antalgol or "Cluster" type. Its period is the shortest of any known variable, the maxima following each other at intervals of 3h 14m 12s.28. There are indications of systematic differences between the odd- and even-numbered maxima so that the period is possibly double that just given. The following formula is sufficiently exact to predict both odd and even maxima.

J.D. 2416563.411 + 0d.1348643 E, Gr. M. T.

The position of the variable as measured on two photographic plates is for 1900.0 R.A. 20h 01m 18s.28. Decl. + 58° 40′ 18″.0

New Variable Stars 80-85.1906 Aquilæ.—In *A.N.* 4108 Professor Max Wolf of Heidelberg calls attention to six new variables found in the vicinity of Miss Fleming's Nova (104.1905) Aquilæ. The following are their positions for 1855 and the range of variation noticed on three photographs taken Sept. 17 and Oct. 16, 1905 and June 26, 1906:

No.	R. A. h m s	Dec. ° ′	Variation
80.1906	18 51 57	− 5 26.0	12 − <15
81.1906	18 52 14	− 7 31.1	12.5 − <15
82.1906	18 54 12	− 9 16.3	13 − 14.5
83.1906	18 55 32	− 7 21.9	10 − 13
84.1906	18 59 44	− 8 26.9	11 − 13
85.1906	19 02 03	− 5 36.5	12.5 − 15

Two New Variables 86. and 87.1906.—These are announced in *A. N.* 4110 by Professor W. Ceraski and were discovered by Mme. L. Ceraski on the Moscow photographs. Their positions for 1900 are

	R. A.			Decl.	
	h	m	s	°	′
86.1906 Bootis	14	43	15	+39	43.7
87.1906 Draconis	16	33	51	+58	01.3

The first appears to vary between 9.0 and 10.0 magnitude but the period is unknown. The second has a short period but the data are insufficient to determine it. Judging from 23 plates taken in 1904-06 the brightness varies between 9.6 and 10.8 magnitude.

New Variable 88.1906 Lacertæ.—In *A.N.* 4118 Professor F. H. Sears of the Laws Observatory, Columbia, Mo., announces that the star BD.+55° 2817, which has been used as a comparison star for V Lacertæ, is itself variable. It has a range of about 0.4 of a magnitude and a period of 5½ and 6 days. The position for 1900.0, as derived from the Helsingfors A. G. Catalogue is

R.A. $22^h\ 44^m\ 58^s.37$ Decl. + 55° 53′ 58″.4

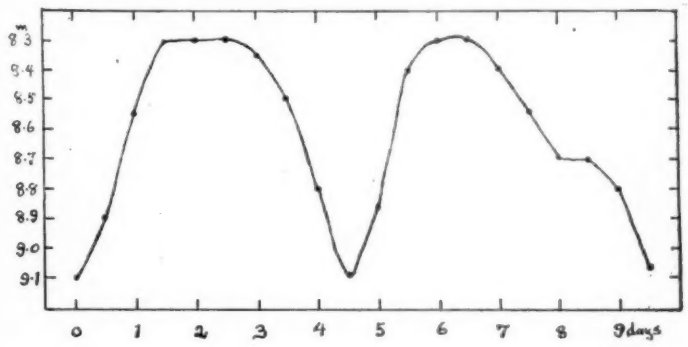
The magnitude given in the BD. is 8.7

Mr. Sears also states that the period of V Lacertæ is almost exactly 5 days.

Variable RW Cassiopeiæ.—Mr. Blajko finds this star to vary between 8.6 and 9.4 magnitude in a period of 14.80 days. He gives the following epochs from which to reckon the times of maxima and minima:

Maximum J.D. 2417062.5 Greenwich M.T.
Minimum J.D. 2417071.5 " "

Variable VZ Cygni.—In *A.N.* 4108 Mr. S. Blajko says that this variable has two maxima of magnitude 8.3 and two minima of magnitude 9.1. The



LIGHT CURVE VZ CYGNI.

minima are sharper than the maxima and are at intervals of 5.0 and 4.727 days. The total period is therefore 9.727 days. The formulæ are

Minima I: J.D. 2417060.1 + 9^d.727 E
Minima II: J.D. 2417065.1 + 9 .727 E.

RZ Ophiuchi.—Mr. Blajko's observations in 1905 and 1906 indicate that this star is of the Algol type. If so it has the longest period of any known Algol variable, the period being either 130.9 or 261.8 days. The minimum brightness is about 10.4 magnitude and remains constant for at least eight days. The rise to the normal brightness, 9.3 magnitude, occupies about two days.

Approximate Magnitudes of Variable Stars on Sept. 1, 1906.

[Communicated by the Director of Harvard College Observatory, Cambridge, Mass.]

Name.	R. A. 1900. h	m	Decl 1900. °	′	Magn.	Name.	R.A. 1900. h	m	Decl. 1900. °	′	Magn.
X Androm	0	10.8	+46	27	8.5 *i*	Z Scorpii	16	0.1	−21	28	9.9*d*
T Androm		17.2	+26	26	9.3*d*	R Herculis		1.7	+18	38	10.5*d*
T Cassiop.		17.8	+55	14	8.2*d*	RR Herculis		1.5	+50	46	8.0
R Androm.		18.8	+38	1	9.7*d*	U Serpentis		2.5	+40	12	12.5*d*
U Cassiop.		40.8	+47	43	9 0 *i*	X Scorpii		2.7	−21	16	14
V Androm.		44.6	+35	6	9.0 *i*	W Scorpii		5.9	−19	53	13 *i*
RR Androm.		45.9	+33	50	10.5 *i*	RX Scorpii		5.9	−24	38	14
W Cassiop.		49.0	+58	1	9.3*d*	RU Herculis		6.0	+25	20	13.2*d*
U Androm.	1	9.8	+40	11	11	R Scorpii		11.7	−22	42	10.8
S Cassiop		12.3	+72	5	8.3*d*	S Scorpii		11.7	−22	39	11.2 *i*
RU Androm.		32.8	+38	10	10	W Coronae		11.8	+38	3	8.8 *i*
X Cassiop		49.8	+58	46	10.5 *i*	W Ophiuchi		16.0	− 7	28	13.8
U Persei		52.9	+54	20	8.6*d*	Y Scorpii		23.8	−19	13	12
R Arietis	2	10.4	+24	35	10.0*d*	V Ophiuchi		21.2	−12	12	9.5*d*
W Androm.		11.2	+43	50	9.5	U Herculis		21.4	+19	7	10.5*d*
o Ceti		14.3	− 3	26	9.0*d*	SS Herculis		28.0	+ 7	3	10.7 *i*
S Persei		15.7	+58	8	10.0	T Ophiuchi		28.0	−15	55	12.5*d*
R Trianguli		31.0	+33	50	8.0 *i*	S Ophiuchi		28.5	−16	57	9.0 *i*
T Arietis		42.8	+17	6	9.0	W Herculis		31.7	+37	32	12.5*d*
W Persei		43.2	+56	34	10.0 *i*	R Draconis		32.4	+66	58	11.0 *i*
Y Persei	3	20.9	+43	50	10 *i*	RR Ophiuchi		43.2	−19	17	10.0*d*
X Camelop.	4	32.6	+74	56	<11.5	S Herculis		47.4	+15	7	11.6*d*
S Camelop.	5	30.2	+68	45	8.5 *i*	RV Herculis		56.8	+31	22	9.9*d*
Y Draconis	9	31.1	+78	18	9.0 *i*	R Ophiuchi	17	2.0	−15	58	13.0*d*
R Urs. Maj.	10	37.6	+69	18	13.5*d*	RT Herculis		6.8	+27	11	9.7*d*
R Comae	11	59.1	+19	20	10.0*d*	Z Ophiuchi		14.5	+ 1	37	8.1 *i*
T Urs. Maj.	12	31.8	+60	2	8.4*d*	RS Herculis		17.5	+23	1	13.5*d*
RS Urs. Maj.		34.4	+59	3	10 *i*	RS Ophiuchi		44.8	− 6	40	11
S Urs. Maj.		39.6	+61	38	12.3*d*	RT Ophiuchi		51.7	+11	10	11.0 *i*
T Urs. Min.	13	32.6	+73	56	13.5	T Draconis		54.8	+58	14	9.7 *i*
R Can. Ven.		44.6	+40	2	11.5*d*	RY Herculis		55.4	+19	29	13
Z Bootis	14	1.7	+13	58	13 *i*	V Draconis		56.3	+54	53	10.5 *i*
S Bootis		19.5	+54	16	11.0 *i*	RW Herculis	18	1.7	+22	4	13.5
V Bootis		25.7	+39	18	8.0	T Herculis		5.3	+31	0	8.6*d*
R Camelop.		25.1	+84	17	8.6 *i*	W Draconis		5.4	+65	56	13.5*d*
R Bootis		32.8	+27	10	8.2 *i*	X Draconis		6.8	+66	8	13 *d*
U Bootis		49.7	+18	6	12 *d*	W Lyrae		11.5	+36	38	12.0*d*
RT Librae	15	0.8	−18	21	14	T Serpentis		23.9	+ 6	14	13
T Librae		5.0	−19	38	<13.5	RZ Herculis		32.7	+25	58	14.2
Y Librae		6.4	− 5	38	12.8	X Ophiuchi		33.6	+ 8	44	7.3*d*
S Librae		15.6	−20	2	9.0*d*	RY Lyrae		41.2	+34	34	<14
S Serpentis		17.0	+14	40	8.8 *i*	RW Lyrae		42.2	+43	32	13
S Coronae		17.3	+31	44	11.5*d*	RX Lyrae		50.4	+32	42	<14
S Urs. Min.		33.4	+78	58	8.0 *i*	Nova Sagitt.		56.2	−13	18	<14.5
R Coronae		44.4	+28	28	6.0	Nova Aquil No. 2		54.4	− 4	39	<14
—Coronae		45.3	+37	49	8.5 *i*	RT Lyrae		57.8	+37	22	11
R Serpentis		46.1	+15	26	8.4*d*	R Aquilae	19	1.6	+ 8	5	11.0*d*
V Coronæ		46.0	+39	52	10.5*d*	V Lyrae		5.2	+29	30	<13.5
R Librae		47.9	−15	56	13.3	RX Sagittarii		8.7	−18	59	11.0*d*
RR Librae		50.6	−18	1	8.5 *i*	RW Sagittarii		8.1	−19	2	10.2*d*

Approximate Magnitudes of Variable Stars on Sept. 1, 1906.

(Continued.)

Name.	R. A. 1900.	Decl. 1900.	Magn.	Name.	R. A. 1900.	Decl. 1900.	Magn.
	h m	° ′			h m	° ′	
S Lyrae	19 9.1	+25 50	12	U Capric.	20 42.6	−15 9	<12.5
RS Lyrae	9.3	+33 15	13 *d*	V Delphini	43.2	+18 58	<13.5
U Draconis	9.9	+67 7	9.0	T Aquarii	44.7	−5 31	13.2*d*
W Aquilae	10.0	− 7 13	11	RZ Cygni	48.5	+46 59	12.0*d*
T Sagittarii	10.5	−17 9	10.5*d*	X Delphini	50.3	+17 16	<12
R Sagittarii	10.8	−19 29	12.2*d*	R Vulpeculae	59.9	+23 26	9.5*d*
RY Sagittarii	10.0	−33 42	7.5	V Capric.	21 1.8	−24 19	12.5
S Sagittarii	13.6	−19 12	13.5*d*	TW Cygni	1.8	+29 0	<12
Z Sagittarii	13.8	−21 7	13 *d*	X Capric	2.8	−21 45	<13.5
TZ Cygni	13.4	+50 0	10	X Cephei	3.6	+82 40	13.5
U Lyrae	16.6	+37 42	10.5	RS Aquarii	5.8	− 4 27	9.5 *i*
T Sagittae	17.2	+17 28	9.8	Z Capric.	5.0	−16 35	10
TY Cygni	29.8	+28 6	10.7 *i*	R Equulei	8.4	+12 23	11.5*d*
RT Aquilae	33.3	+11 29	10.0	T Cephei	8.2	+68 5	9.5*d*
R Cygni	34.1	+49 58	12.2*d*	RR Aquarii	9.8	− 3 19	9.8 *i*
RV Aquilae	35.9	+ 9 42	9.6 *i*	X Pegasi	16.3	+14 2	12.2*d*
RT Cygni	40.8	+48 32	11.5*d*	T Capric.	16.5	−15 35	11.5 *i*
X Aquilae	46.5	+ 4 12	13.5	Y Capric.	28.9	−14 25	<13.5
TU Cygni	43.3	+48 49	<13.5	S Cephei	36.5	+78 10	8.8 *i*
χ Cygni	46.7	+32 40	12.1*d*	RU Cygni	37.3	+53 52	9.0
RR Aquilae	52.4	− 2 11	13	SS Cygni	38.8	+43 8	8.3*d*
RS Aquilae	53.7	− 8 10	9.5	RR Pegasi	40.0	+24 33	<13.5
Z Cygni	58.6	+49 46	8.8 *i*	V Pegasi	56.0	+ 5 38	14
S Cygni	20 3.4	+57 42	<14	U Aquarii	57.9	−17 6	11.8
R Capric.	5.7	−14 34	<12	RT Pegasi	59.8	+34 38	12 *d*
S Aquilae	7.0	+15 19	11.5	T Pegasi	22 4.0	+12 3	13
RU Aquilae	8.0	+12 42	<13	Y Pegasi	6.8	+13 52	<13
W Capric.	8.6	−22 17	10.5 *i*	RS Pegasi	7.4	+14 4	9.0
Z Aquilae	9.8	− 6 27	13.5	X Aquarii	13.2	−21 24	8.5
RS Cygni	9.8	+38 28	9.0*d*	RT Aquarii	17.7	−22 34	<12
R Delphini	10.1	+ 8 47	11.0 *i*	T Lacertae	17.9	+33 52	10.3
RT Capric.	11.2	−21 38	7.5	S Aquarii	24.6	+39 48	10.0*d*
SX Cygni	11.6	+30 46	<12	R Aquarii	38.8	+41 51	14.3*d*
WX Cygni	14.8	+37 8	10.8	RW Pegasi	59.2	+14 46	12
V Sagittae	15.8	+20 47	12.8	S Aquarii	51.8	−20 53	8.8 *i*
U Cygni	16.5	+47 35	7.3 *i*	R Pegasi	23 1.6	+10 0	7.5 *i*
RW Cygni	25.2	+39 39	8.8*d*	V Cassiop.	7.4	+59 8	11.0*d*
RU Capric.	26.7	−22 2	<12	W Pegasi	14.8	+25 44	12.0 *i*
ST Cygni	29.9	+54 38	11 *i*	S Pegasi	15.5	+ 8 22	8.3 *i*
Y Delphini	36.8	+11 30	10.0 *i*	R Aquarii	38.6	−15 50	10.0 *i*
S Delphini	38.5	+16 44	11.7*d*	Z Aquarii	47.1	−16 25	8.0
V Cygni	38.1	+47 47	10.5	RR Cassiop.	50.6	+53 10	11.5 *i*
Y Aquarii	39.2	− 5 12	13.5	R Cassiop	53.3	+50 50	9.6*d*
T Delphini	40.7	+16 2	11.5*d*	Z Pegasi	55.0	+25 21	12
V Aquarii	41.8	+ 2 4	9.0*d*	Y Cassiop	58.2	+55 7	14
W Aquarii	41.2	− 4 27	9.5*d*				

The letter *i* denotes that the light is increasing, the letter *d* that the light is decreasing, the sign <, that the variable is fainter than the appended magnitude.

The magnitudes given have been compiled by Mr. Leon Campbell of the Harvard College Observatory, from observations made at the Vassar College, Whiteside, and Harvard Observatories.

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Greenwich Mean Time beginning with noon. To reduce to Central Standard time subtract 6 hours, or for Eastern time subtract 5 hours.]

Minima of Variable Stars of the Algol Type.—Continued.

U Cephei

	d	h
Nov.	2	20
	5	7
	7	19
	10	7
	12	19
	15	7
	17	19
	20	6
	22	18
	25	6
	27	18
	30	6

Z Persei

	d	h
Nov.	1	15
	4	16
	7	17
	10	19
	13	20
	16	21
	19	23
	23	0
	26	1
	29	3

Algol

	d	h
Nov.	1	1
	3	22
	6	19
	9	16
	12	13
	15	10
	18	6
	21	3
	24	0
	26	20
	29	18

RT Persei

	d	h
Nov.	1	19
	2	15
	3	11
	4	8
	5	4
	6	0
	6	21
	7	17
	8	14
	9	10
	10	6
	11	3
	11	23
	12	20
	13	16
	14	12
	15	9
	16	5
	17	2
	17	22
	18	18
	19	15
	20	11
	21	7

RT Persei

	d	h
Nov.	22	4
	23	0
	23	21
	24	17
	25	13
	26	10
	27	6
	28	3
	28	23
	29	19
	30	16

λ Tauri

	d	h
Nov.	3	6
	7	5
	11	4
	15	2
	19	1
	23	0
	26	23
	30	22

R Canis Maj.

	d	h
Nov.	1	9
	2	12
	3	16
	4	19
	5	22
	7	1
	8	5
	9	8
	10	11
	11	15
	12	18
	13	21
	15	0
	16	4
	17	7
	18	10
	19	13
	20	17
	21	20
	22	23
	24	2
	25	6
	26	9
	27	12
	28	15
	29	18
	30	22

RR Puppis

	d	h
Nov.	1	18
	8	4
	14	15
	21	1
	27	11

V Puppis

	d	h
Nov.	2	2
	3	13
	5	0
	6	11
	7	22

V Puppis

	d	h
Nov.	9	9
	10	20
	12	7
	13	17
	15	4
	16	15
	18	2
	19	13
	21	0
	22	11
	23	22
	25	9
	26	20
	28	7
	29	17

S Cancri

	d	h
Nov.	7	18
	17	6
	26	18

S Velorum

	d	h
Nov.	4	5
	10	3
	16	1
	22	0
	27	22

RR Velorum

	d	h
Nov.	2	13
	4	9
	6	6
	8	2
	9	23
	11	19
	13	16
	15	12
	17	9
	19	5
	21	2
	22	22
	24	19
	26	15
	28	12
	30	8

Z Draconis

	d	h
Nov.	2	5
	3	13
	4	22
	6	7
	7	15
	9	0
	10	8
	11	17
	13	1
	14	10
	15	19
	17	3
	18	12
	19	20
	21	5
	22	13
	23	22

Z Draconis

	d	h
Nov.	25	6
	26	15
	28	0
	29	8
	30	17

Z Herculis

	d	h
Nov.	3	8
	5	5
	7	7
	9	4
	11	7
	13	4
	15	7
	17	4
	19	7
	21	4
	23	7
	25	4
	27	7
	29	4

RS Sagittarii

	d	h
Nov.	1	2
	3	12
	5	22
	8	8
	10	18
	13	4
	15	14
	18	0
	20	10
	22	20
	25	6
	27	16
	30	2

V Serpentis

	d	h
Nov.	2	11
	5	22
	9	9
	12	19
	16	6
	19	17
	23	4
	26	15
	30	2

RX Herculis

	d	h
Nov.	1	7
	2	5
	3	2
	3	23
	4	21
	5	18
	6	15
	7	13
	8	10
	9	7
	10	5
	11	2
	11	23
	12	21
	13	18

RX Herculis

	d	h
Nov.	14	15
	15	13
	16	10
	17	7
	18	5
	19	2
	20	0
	20	21
	21	18
	22	15
	23	13
	24	10
	25	7
	26	5
	27	2
	27	23
	28	21
	29	18
	30	15

RR Draconis

	d	h
Nov.	1	11
	4	7
	7	3
	9	23
	12	19
	15	15
	18	11
	21	7
	24	3
	26	23
	29	19

RV Lyræ

	d	h
Nov.	3	20
	7	10
	11	0
	14	15
	18	5
	21	19
	25	10
	29	0

U Sagittæ

	d	h
Nov.	4	5
	7	14
	10	23
	14	9
	21	3
	24	12
	27	21

SY Cygni

	d	h
Nov.	6	17
	12	17
	18	17
	24	17
	30	18

WW Cygni

	d	h
Nov.	3	20
	7	4
	10	11
	13	19
	17	3

Minima of Variable Stars of the Algol Type.—Continued.

WW Cygni		VW Cygni		W Delphini		Y Cygni		VV Cygni	
	d h		d h		d h		d h		d h
Nov.	20 10	Nov.	30 18	Nov.	24 7	Nov.	21 0	Nov.	14 1
	23 18	UW Cygni			29 2		22 13		15 13
	29 2	Nov.	1 3	Y Cygni			24 0		17 0
	30 9		4 13	Nov.	1 14		25 13		18 12
SW Cygni			8 0		3 0		27 0		19 23
Nov.	2 2		11 11		4 14		28 13		21 11
	6 16		14 22		6 0		30 0		22 22
	11 5		18 9		7 14	VV Cygni			24 10
	15 19		21 19		9 0	Nov.	2 6		25 21
	20 9		25 6		10 14		3 17		27 8
	24 23		28 17		12 0		5 5		28 20
	29 12	W Delphini			13 13		6 16		30 7
VW Cygni		Nov.	5 1		15 0		8 4	UZ Cygni	
Nov.	5 19		9 21		16 13		9 15	Nov.	13
	13 21		14 16		18 0		11 3		
	22 8		19 11		19 13		12 14		

Maxima of Variable Stars of Short Period not of the Algol Type.

The times of maxima only are given; the times of minima may be obtained by subtracting the interval printed in parentheses under the name of the star.

Y Aurigæ		W Carinæ		W Virginis		β Lyræ		η Aquilae	
	d h		d h		d h		d h		d h
	(−0 18)		(−1 0)		(−8 5)		{−3 7}		(−2 6)
Nov.	4 7	Nov.	2 10	Nov.	11 21		{−3 2}	Nov.	3 9
	8 4		6 19		20 3	Nov.	3 17		10 13
	12 0		11 4	X Sagittarii			9 23		17 17
	15 21		15 13		(−2 21)		16 15		24 21
	19 18		19 22	Nov.	1 3		22 21	S Sagittae	
	23 14		24 7		8 3		29 13		(−3 10)
	27 11		28 16		15 3	κ Pavonis		Nov.	2 13
T Monocerotis		S Muscæ			22 3		(−4 7)		10 22
	(−7 22)		(−3 11)		29 4	Nov.	5 13		19 7
Nov.	6 5	Nov.	5 16	Y Ophiuchi			14 15		27 16
W Geminorum			15 18		(−6 5)		23 17	X Cygni	
	(−2 16)		25 0	Nov.	12 13	U Aquilæ			(−6 19)
Nov.	1 17	T Crucis			29 16		(−2 4)	Nov.	16 14
	9 11		(−2 2)	W Sagittarii		Nov.	2 6	Dec.	3 0
	17 4	Nov.	4 6		(−3 0)		9 7	T Vulpeculae	
	24 22		10 23	Nov.	7 5		16 7		(−1 10)
ζ Geminorum			17 17		14 19		23 8	Nov.	3 11
	(−5 0)		24 10		22 10		30 8		7 22
Nov.	9 21	R Crucis			30 0	U Vulpeculae			12 8
	20 1		(−1 10)	Y Sagittarii			(−2 3)		16 19
	30 4	Nov.	5 10		(−2 2)	Nov.	5 0		21 5
V Carinæ			11 6	Nov.	3 21		12 23		25 16
	(−2 4)		17 2		9 15		20 23		30 2
Nov.	2 9		22 22		15 10		28 22	WZ Cygni	
	9 1		28 17		21 4	SU Cygni		Minimum	
	15 18	S Crucis			23 23		(−1 8)	Period 14[h]	
	22 11		(−1 12)	U Sagittarii		Nov.	2 22	Nov.	1 9
	29 3	Nov.	2 6		(−2 23)		6 18		2 13
T Velorum			6 23	Nov.	3 9		10 14		3 17
	(−1 10)		11 15		10 3		14 11		4 21
Nov.	3 16		16 8		16 21		18 7		6 1
	8 7		21 0		23 15		22 3		7 5
	12 23		25 17		30 8		26 0		8 9
	17 14		30 9				29 20		9 13
	22 5								10 17
	26 21								

Maxima of Variable Stars of Short Period not of the Algol Type.

(Continued)

WZ Cygni		WZ Cygni		WZ Cygni		VY Cygni		δ Cephei	
	d h		d h		d h		d h		d h
Nov.	11 21	Nov.	20 2	Nov.	28 6	Nov.	3 0		(−1 10)
	13 1		21 6		29 10		10 21	Nov.	3 16
	14 6		22 10		30 14		18 17		9 1
	15 10		23 14	TX Cygni			26 14		14 9
	16 14		24 18	(−5 3)					19 18
	17 18		25 22	Nov.	13 21				25 3
	18 22		27 2		28 15				30 12

Variable Star Notes.

W LYRÆ

The comparison stars used in the following record of the recent minimum and maximum of this star are those in POPULAR ASTRONOMY for October 1897, page 328.

1906 January 3 The variable is less than e, equals p.
19 Less than t, equals u. The latter is slightly dimmer than t. Morning clear.
24 The same.
February 1 W is scarcely discernible. Probably of about 12 magnitude.
11 It is just discernible.
28 Invisible. Morning rather clear.
March 7 Invisible. Morning clear.
19 The same. Morning clear.
31 11:40 p. m. Visible but dimmer than t. Low altitude but night clear.
April 4 Much dimmer than g, less than t, equal to u. High altitude. Night clear.
12 Less than g, equal to t, brighter than u.
May 5 Nearly equal to e, less than a. Gibbous Moon southwest.
11 The same.
16 Equal to e, less than a.
17 The same. Very clear.
19 and 20 The same. Clear.
27 Equal to a, less than n.
June 5 Rather brighter than a.
9 Very slightly dimmer than a.
12 Equals a, brighter than e, less than n.
18 The same.
20 Less than a or n, brighter than e. Night very clear.
26 Scarcely brighter than e.
July 5 Less than e, brighter than g.
16 Equal to g and f.
21 The same.
31 Less than g, very nearly equal to t.

V CORONÆ

POPULAR ASTRONOMY for August and September 1904 contains, on page 496, a map of the neighborhood of this variable on which the comparison stars, used in the following record, are lettered.

1906 January 24 Brighter than f or e, less than d.
March 27 Brighter than c and about two steps brighter than b. It is reddish. Night fairly clear.
31 Scarcely two steps brighter than b.
April 10 b, c and the variable seem equal in moonlight.
27 Between c and b.

May	11	Equal to c.
	16	Less than c, but nearer to c than to e.
	20	Between d and e.
	28	Less than c brighter than d.
June	9	Nearer to c than to d.

U GEMINORUM

During the present year only negative observations were obtained of this irregular variable, which at least show that on the dates given below it was not of sufficient magnitude to be visible in a four inch refractor. Among the closely adjacent stars, f of 11.2 magnitude was always seen on these occasions.

1906 January	2	9:10. p. m.
	3	9: p. m.
	19	9:50. p. m. g, of 12 magnitude visible.
	23	8:40 p. m.
	29	8:10. p. m. g, visible.
	30	8:25. p. m.
	31	8:40. p. m.
February	11	9:05. p. m.
	25	7:10. p. m..

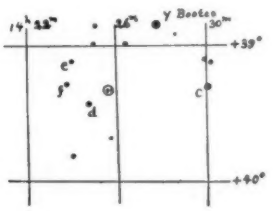
The positions of the comparison stars are shown on a map of the vicinity of U Geminorum in POPULAR ASTRONOMY May 1897, page 19.

SS CYGNI

A few estimates of the brightness of this irregular variable were made by comparison with the stars around it, the magnitudes of which have been accurately ascertained by Mr. Parkhurst and other observers.

1906 April	4	2:30 a. m. Of about 10.5 magnitude.
June	20	9:35 p. m. 10.6 magnitude.
July	16	9:05 p. m. 11 magnitude.
	21	9:30 p. m. of nearly 9 magnitude.

V BOÖTIS



VICINITY OF V BOÖTIS.

1905 March	26	The variable is brighter than any of the semi-circle stars near it. It equals c.
April	4	The same.
	27	Less than c, equals d.
May	4	The same.
	21	Less than d, equals e, brighter than f.
1906 March	18	Between e and f.
	27, 31	The same.
April	4	The same.
	10	Nearer to the brightness of e than to f.
	27	Equals e.

May 11 Equals d. Brighter than e.
16 The same.
27 Brighter than d, less than c.

ROSE O'HALLORAN.

San Francisco,
August 9, 1906.

GENERAL NOTES.

During a week in August we visited the Yerkes Observatory, Williams Bay, Wis., and were the guest of Professor and Mrs. E.E. Barnard. We were strongly and favorably impressed with the amount of good work now going on in all the varied lines of research undertaken at this great Observatory.

Some of the articles of this number would have appeared earlier if we could have given space for them sooner. The same may be said of other important matter. Our increased size has not entirely relieved the pressure that has been upon us for months past, to give more useful popular reading.

Radium. No single element in the whole list known in nature has been so much talked about recently in scientific circles as radium. Many interesting and true things have been learned about it, and these have been fully described in lectures and current periodicals. We notice that Lord Kelvin has given timely warning against accepting the views of some who hold to the principle that there is a gradual evolution of one element into another. He claims that such a thing has not yet been proved. The apparent transformation of radium into helium is yet only apparent. It does not necessarily follow though helium may be obtained from radium that actual transformation of these elements has taken place. Our physicists must be painstakingly cautious about large conclusions from narrow generalizations.

The Use of the Spectroscope. In the April number of the *British Journal of Photography* (p. 307) is found one of the best brief articles on the elementary use of the spectroscope that we have seen in print for many a day. It was written by E. J. Wall, a practical spectroscopist, who evidently well understands the instrument of which he speaks, and more than that, knows how to tell what he knows so that a beginner can use the information given easily and certainly. The article is worthy of reproduction, for the sake of many readers of this magazine who would be greatly helped by it in the beginning of this most delightful study and it is given on page 336.

Observations of Algol. An interesting study of Algol is given in number 6 of volume 16 of the *Journal of the British Astronomical Association* p. 230 by E. E. Marwick, covering a period from 1899 to 1906. The paper is accompanied by a double page plate, neatly and plainly showing the light curve of the star, noting the observations that are first class, and those of the second and third classes, observations of the same phase, the mean brightness, and lines to show mean and probable light curve and scale in hours of change. The article is a very useful one.

Correction of Hansen's Table for the Moon by Mr. Cowell. In the March issue of *Monthly Notices*, is a most critical review of Mr. Cowell's series of papers which he has been contributing for the last two years to the same publication, on the errors of the lunar tables, as shown by the Greenwich Observations since 1750. This important review is by E. Nevill who has been working for thirty years on the same subject, and, while he regards Mr. Cowell's work "as one of the most important contributions that have been made for years to the better understanding of the motion of the Moon," he offers some criticism on the results obtained by him.

Mr. Nevill's paper covers thirty-eight pages of printed matter in the copy of *Monthly Notices* referred to and raises six points for consideration.

The first is regarding the methods of investigation. They are claimed to be "perfectly sound." Some small and important errors in work were noticed. The criticisms offered are upon the proper interpretation to be put on the results deduced from an analysis of the observations.

2. The first step in the investigation of the origin of the observed errors of the lunar tables is to consider the bearing of the results obtained by theory by M. Radan. Mr. Cowell's work in regard to these theoretical results does not seem quite clear.

3. The next step deals with the actual corrections to the lunar tables deduced by Mr. Cowell from his discussion of the observations. This point covers three things; (1) His corrections to the secular and empirical inequalities of very long period.

(2) His values for the coefficients of the inequalities arising from the disturbing action of the planets and

(3) His corrections for the tabular coefficients of the perturbations due to the direct action of the Sun.

4. He deals with secular terms and semi-secular terms of long period.

5. Shorter periods are considered which depend on observations from 1750 to 1900.

6. In this part of the paper a comparison is made between the values of the terms of the longer period, deduced by Mr. Cowell from observations with the standard observed errors.

This thorough review and kindly criticism in no way seriously impairs the splendid work of Mr. Cowell already previously referred to in this magazine.

The Kodaikanal Solar Physical Observatory. The establishment of this new solar physical Observatory by the government of India makes a step in advance in astronomical work especially for the needs of meteorology. This important step has been under consideration for more than twenty years and now the hopes of its persistent friends are already being realized.

We were glad to see in a recent number of the *Monthly Weather Review*, an interesting article by the librarian of the Washington Weather Bureau, Herbert H. Kendall, setting out quite fully the plan and purpose of this new Solar Observatory. The supply of instruments is quite complete, and the plan of work embraces four things:

Solar Physics.

(a) Observations of the six most widened lines in sun-spot spectra between *F* and *b* and other six between *b* and *D*.

(b) Observations of other widened sun-spot spectra.

(c) Visual Observations of prominences and chromosphere.

(d) Photographs of the solar disk in monochromatic light.

(e) Photographs of sun-spots.

Meteorological Observations.

(f) Complete

Other Observations.

(g) Actinometry.

(h) Earthquake records.

(i) Cloud observations.

Magnetic Observations.

The publications of this Observatory will form an important adjunct to the reference library from this time forward.

Observations of Phoebe in May and June 1906. Six photographs of Saturn, showing images of Phoebe, have been obtained with the 24-inch Bruce Telescope at Arequipa. They are given in Table I. in a form nearly the same as that contained in Circular 109, for the similar observation of 1905. The designation of the plate, the date, the Greenwich Mean Time of the middle of the exposure, and the duration of the exposure are given in the first four columns. The difference in right ascension, the difference in declination, the distance, and the position angle are given in the fifth, sixth, seventh, and eighth columns. Of these, the sixth and seventh are the measured quantities, the fifth and eighth are derived from them by computation. Computed values for the differences in right ascension and declination were taken from the ephemeris given on the leaf following the Contents in the second edition of the American Ephemeris and Nautical Almanac for 1906, and subtracted from the observed values in the fifth and sixth columns. The residuals are given in the ninth and tenth columns. The measures were made by Mr. E. S. Manson, Jr.

TABLE I.

POSITIONS OF PHOEBE. 1906.

Plate	Date 1906	G.M.T.		Exp.	Difference in R.A.		Difference in Dec.	Distance	P.A.	O—C. R.A.	O—C. Dec.
		h	m	m	m	s	′	′	°	s	′
A 7674	May 18	21	3	120	−1	34.1	−10.8	25.7	245.0	−0.5	−0.2
A 7676	" 19	21	7	120	−1	33.8	−11.0	25.7	244.7	−0.1	−0.4
A 7745	June 25	19	0	120	−1	26.8	− 9.0	23.3	247.3	−1.0	+0.1
A 7754	" 26	18	55	120	−1	26.3	− 9.0	23.2	247.2	−1.0	0.0
A 7758	" 27	18	59	120	−1	25.4	− 9.0	23.0	247.1	−0.7	0.0
A 7763	" 28	18	53	121	−1	25.0	− 9.0	22.9	246.9	−0.8	−0.1

If desired, more precise measures of these photographs can be made. As the uncertainty in each coördinate rarely exceeds $0'.1 = 6''$, it does not seem as if this would be necessary, at least until elements are determined which will represent the earlier positions more closely.

EDWARD C. PICKERING.

August 19, 1906.
Harvard College Observatory.
Circular 118.

The Elements of the Double Star β 733 = 85 Pegasi. — In *A. N.* 4110 Mr. W. Doberck gives the following elements and ephemeris of the double star 85 Pegasi, determined by the use of 67 measures made by various observers from 1878 to 1904.

ELEMENTS OF 85 PEGASI.

$\Omega = 106°\ 20'$	$P = 25.3\epsilon$ years
$\lambda = 267\ \ 54$	$T = 1908.38$
$\gamma = \ 52\ \ 50$	$a = 0''.841.$
$e = 0.3872$	

EPHEMERIS.

	Pos. Ang. °	Dist. ″		Pos. Ang. °	Dist. ″
1906.7	308.5	0.521	1909.7	71.8	0.448
1907.7	338.1	0.367	1910.7	92.4	0.602
1908.7	31.0	0:320	1911.7	105.3	0.716

Elements of 70 *p* Ophiuchi.—In *A. N.* 4115 Mr. W. Doberck of the Hong Kong Observatory gives a new determination of the elements of the orbit of this double star, using all the available observations from 1779 to 1905. The components of this binary are of magnitudes 4.5 and 6, colored yellow and purple. It was discovered by Sir William Herschel in 1779 and has completed nearly one and one-half revolutions since that time. The period is about 87.5 years.

ELEMENTS OF 70 *p* OPHIUCHI.

$\Omega = 122° 22'$
$\lambda = 165\ 50$ 1900.0
$\gamma = 59\ 08$
$e = 0.4989$

$P = 87.49$ years
$T = 1896.04$
$a = 4''.543$
Motion retrograde.

EPHEMERIS.

Epoch	Pos.Ang. °	Dist. ″	Epoch	Pos.Ang. °	Dist. ″
1906.60	169.5	2.39	1916.60	138.2	4.88
07.60	163.6	2.66	17.60	136 6	5.07
08.60	158.7	2.94	18.60	135.2	5.25
09.60	154.7	3.21	19.60	133.9	5.42
10.60	151.3	3.48	20.60	132.6	5.58
11.60	148.4	3.74	21.60	131.4	5.73
12.60	145.9	3.99	22.60	130.3	5.87
13.60	143.7	4.22	23.60	129.2	5.99
14.60	141.5	4.45	24.60	128.2	6.11
15.60	139.8	4.67	25.60	127.2	6.21

Star Charts. Professor F. H. Sears, Director of Laws Observatory, University of Missouri has called our attention to a need he feels of a series of star charts for use in the study of the constellations. He refers to existing helps as both too bulky and too expensive. In a private letter under date of September 17, he says: "The charts printed by you in Popular Astronomy each month, if collected together for the year, would be just the thing; and I have wondered if you would be disposed to entertain a proposition which would make these charts available for class-work. It would be sufficient to print the twelve charts, on rather heavy paper, and saddle stitch them into a heavy manila cover, with possibly a one-page introduction, giving the names of the brighter stars. All this could be done so inexpensively that they could be retailed to the student for a few cents, and would greatly facilitate the work of instruction."

In reply to this suggestion of Professor Sears, we would say that we have been doing this thing for some Professors of Astronomy in Colleges and Universities desiring such aid for several years past. His suggestion to have a list of the names of the principal stars in connection with the star charts is important. Possibly others desiring such an inexpensive pamphlet, either as a teacher or a private student, might wish a little something more. If the call for such a pamphlet should be large enough the cost could be correspondingly reduced.

The Sidereal Year. A new, a striking and an unexpected application of the sidereal day has come into use recently. Our eye happened to notice for the

first time, a few days ago, the title of a small publication which is "The Book of Daniel Unlocked", and being curious about the key that should unlock the numerical mysteries of the prophecies of that book, we at once read it, almost at one sitting, and to our surprise the author claims that the sidereal year, which is 365 days, 6 hours, 9 minutes and 9 6/10 seconds long, the key needed for the perfect and complete understanding of the numerical parts of those great prophecies. We have not yet read the book carefully and critically enough to be sure the author is right so far as the astronomy goes. It will pay us certainly, and possibly others to read that book severely for what light it may throw on the meaning of some prophecy that may now be in the process of fulfillment in our own day.

The length of the sidereal year is a little too long as given above. It should be 365d. 6h. 9m. 8.97s.

It is further stated that the sidereal year has not varied as much as one one-hundredth of a second in 2000 years, which statement the author bases on the authority of Laplace and Herschel. Not too much confidence should be placed in the absolute accuracy of that statement, although from very high authority. Better say, on the average, we do not know that the sidereal year varies in length to that amount. Astronomers believe that there must be some variation in the length of sidereal years on account of the tides and for some other causes, but just exactly how much that variation is, no one yet positively knows. These facts may not at all disturb the conclusions of the author, and yet they are mentioned for what they are worth.

The author of this book is W. S. Auchincloss C. E., and the publishers are D. Van Nostrand Company, 23 Murray Street. New York.

A Durchmusterung of Variable Stars. The plan described below for a Durchmusterung of variable stars has been proposed by Professor Solon I. Bailey.

Certain regions of the sky have now been systematically examined for the discovery of variable stars, the number of which has already increased to more than three thousand. Taking into consideration the immense amount of work which is being carried forward in this line at the Observatories of Harvard, Heidelberg, Moscow, and elsewhere, the right time appears to have come for concerted action among astronomers, looking toward a systematic examination of the whole sky in order to determine the number and distribution of all variable stars, to the faintest magnitudes possible. Otherwise there may be needless duplication in the search, and, as a result, unfortunate waste of labor.

The objection may be made that, if the examination is extended to the whole sky, the number of variables will be so great that their careful observation will be impossible. This may be true; but at least we shall be able to study the distribution of the variable stars, and learn what part they play in the construction of the stellar universe. Since all variables will tend to fall into a few well-defined classes, an examination only sufficient for classification will be necessary for the greater part of them, and elaborate researches will need to be extended only to representative types, and to objects of special interest. A Durchmusterung of variable stars, therefore, would be a worthy achievement for the present generation of observers.

While we recognize the eminent service which has been rendered during the last century by visual observers, it is certain that photographic methods now yield vastly greater results for a given expenditure of time. This is especially true for densely crowded and faint stars. For a study of the whole sky, the

plates should be taken with a telescope of the form known as a photographic doublet, since this gives good definition over a large area. This plan would leave, nevertheless, extended fields of investigation, for those who have instruments of other forms, such as the study of clusters, nebulous regions of small extent, and other special objects. This scheme offers to amateur astronomers an opportunity to take part in a research of permanent value. There are a number of suitable instruments in the possession of those in charge of small observatories, or of amateurs, who can give only limited time to astronomical research; but the thorough and systematic examination of even small portions of the sky would have high value, irrespective of the number of variable stars discovered.

It is undesirable at present to divide the sky into definite areas, and to assign them, by means of some variable-star committee, to the different institutions and observers taking part in such a research. The field is so vast that it will probably be sufficient for each observer to indicate the regions which he prefers to examine. If, after all, a certain amount of duplication takes place, it may be useful, as providing a check on the results obtained by different observers.

For a discussion of the distribution of variable stars it seems absolutely essential that all stars, which surely vary, should be included. No variable should be omitted because its range of variation is so small that it seems to be lacking in interest. While the inclusion of doubtful objects is objectionable, the exclusion of any stars which really vary is still more unwise. Also, all stars which appear on the plates, in the regions selected, should be examined, and the published results should include, if possible, not only the variables found, but also the approximate number of the stars examined. Negative results are next in importance to the discovery of new variables. In a discussion of the distribution, a region void of variables would have nearly the same interest as a rich region like the Small Magellanic Cloud, which contains a thousand variables.

It is hoped that exposures of one or better two hours can be used, and photographic telescopes of eight inches, or more, aperture. This may be expected to show stars as faint as the sixteenth magnitude. An examination of several plates of each region is desirable in order to make the search reasonably complete. The variability should also be confirmed by a second observer.

There should be complete freedom for each observer to use such methods of examination, discussion, and publication, as seem best to him.

Various methods for comparing stellar images on different plates have been used with success. In globular clusters, where the stars are too closely packed to make the method of superposing one plate on another seem feasible, all the stars in a small area were arranged in a sequence in order of brightness, and this sequence was kept in mind while successive plates, usually ten in number, were examined. In the Magellanic Clouds and elsewhere, on the Bruce plates, a positive is made from one negative, on which another negative is superposed, so that bright and dark images of the same stars are seen together. This method is found in practice exceedingly successful and rapid. At some observatories a stereocomparator is employed. Several exposures on the same plate, but taken at different times, may be used. But in order to find variables of long period in this way the exposure should be distributed over several months, at least. When the period is very close to one day, or a submultiple of a day, plates taken at about the same hour on successive nights will fail to show any variation. They must be taken at different hours of the night, or separated by long intervals. Of course, a variable with a period of precisely one day and with a maximum, or minimum, extending throughout the night, would never be discovered by an observer working at a single station. Special methods for detecting variables,

such as observation of bright hydrogen lines in the spectra of stars of the third type, are probably not practicable for such a Durchmusterung.

The number of stars in the sky of the sixteenth magnitude and brighter is not accurately known, but may be estimated at fifty millions. The comparison of such a vast number of stars on several plates is indeed so great an undertaking, that at first it seems impossible. By the rapid methods of photography, however, this work is probably not too extensive for the present generation of variable star observers, provided systematic methods are employed and duplication is avoided. The photographs made for this purpose, also, will be valuable for other lines of astronomical research.

Should this plan of coöperation meet the approval of astronomers, the Harvard Observatory will be glad to undertake its part.

EDWARD C. PICKERING.

May 11, 1906.
Harvard College Observatory.
Circular 116.

Observations of the Crater Linne—during the lunar eclipse of February 8, 1906. "In accordance with the suggestion of Professor W. H. Pickering, in *Popular Astronomy*, the following measures of the diameter of Linné were made at the time of the total lunar eclipse of February 8, 1906. The instrument was the twelve-inch equatorial of this observatory, with filar micrometer, and eyepiece giving a power of 170. Preliminary measures were made on the nights of February 4 and 5, and the method adopted was that used by Professor Pickering, where both micrometer wires are placed on the same side of the object, and the width of the space between the wires is changed until it appears equal to the diameter of the spot.

As a check upon the work, I also measured the small crater, which is about 49″ northwest of Linné, the approximate selenographic latitude and longitude of which are respectively N. 30°.03, and W. 13°.75. This little crater appeared as a well-defined round spot, and will be referred to as Crater 2.

Both spots were measured in a north and south direction, or at right angles to the diurnal motion. Each observed diameter is the result of six settings, that is, three double distances. The results are given as measured, except that the width of a micrometer wire, 0″.48, has been subtracted from the observed diameters. No correction for change of Linné, due to variation in the Moon's age, has been applied, and the reduction to a common distance has been neglected. This latter correction amounts to 0″.1 to be added to the measures of Linné on February 5, but the distances at other dates were so near that at the time of the eclipse, that the correction is less than 0″.1. On account of the large systematic errors possible in measuring so hazy an object, the diameters are measured off to one decimal place. The seeing is given on a scale from 0 to 5, 5 being the best.

The last measure of Linné before the eclipse, was made when the spot was rapidly being lost in the umbra; and again at reappearance the first setting was obtained while Linné was still very dark.

For some time preceding totality the seeing was called 2, or fair; but when Linné had emerged, it was much worse and was called 1. The measures were made as well as possible, and without prejudice until $21^h\ 40^m$, when the settings were compared with those taken before the eclipse, and much to my surprise I found that I was measuring Linné larger by about thirty per cent. I then took another set of Linné and of Crater 2, and could not help noticing an immediate decrease. Rather than go on after I had become prejudiced, and with such bad

seeing, the observations were discontinued. Measures on subsequent dates were made after I had forgotten the data of the eclipse night.

It will be noticed that both Linné and Crater 2 were measured larger in poor than in good seeing, and in my judgment the large apparent increase in the diameters after the eclipse was due almost wholly to this cause. The fact that the change, was greater for Linné, than for Crater 2, is easily explained by the hazy outline of the former. If there was any increase in the size of Linné, as has been reported at the the time of previous eclipses, it was too small to be detected here under the conditions when the following measures were made.

DIAMETERS OF LINNE AND OF CRATER 2.

1906	G. M. T. h	m	Linne ″	Crater 2 ″	Seeing
February 5	15	±	3.0	..	
	..	..	3.0	..	
	..	..	3.2	..	
	..	..	3.4	..	
	Mean		3.2	..	0
February 8	18	52	2 4	..	
	18	57	2.5	..	
	19	7	..	2.1	
	19	11	..	2.0	
	19	14	2.5	..	
	19	17	2.4	..	
	19	20	..	2.1	
	19	22	..	2.1	
	19	26	2.3	..	
	19	29	2.4	..	
	Mean		2.4	2.1	2
Linne Eclipsed.					
February 8	21	19	3.4	..	
	21	22	..	2.4	
	21	24	3.0	..	
	21	28	3.2	..	
	21	31	..	2.2	
	21	34	3.2	..	
	21	36	3.0	..	
	21	40	..	2.5	
	Mean		3.2	2.4	1
	21	49	2.6	..	1
	21	51	..	2.0	1
February 10	16	35	2.6	..	
	16	37	2.7	..	
	16	41	..	1.9	
	16	43	..	1.8	
	16	45	2.5	..	
	16	47	2.6	..	
	16	49	..	2.0	
	16	51	..	2.0	
	Mean		2.6	1.9	2
March 9	14	41	2.2	..	
	14	44	2.1	..	
	14	51	..	1.8	
	14	53	..	1.8	
	14	57	2 2	..	
	15	0	2.1	..	
	15	3	..	1.8	
	15	5	..	1.8	
	Mean		2.2	1.8	2

JOEL H. STEBBINS.

See Remarks.

University of Illinois Observatory,
Urbana, Ill., 1906 March.

Mechanical Solution or Kepler's Equation. The plate which appeared as frontispiece last month was wrongly inserted by the binders, and the error noted too late for rectification. For the convenience of those who may wish to bind this volume of POPULAR ASTRONOMY, we enclose with this number a duplicate of that plate.

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Back volumes or single numbers are still on sale at the old prices, $2.50 and 30 cents, respectively.

WM. W. PAYNE,
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